PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

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April 24, 1902.

Sir WILLIAM HUGGINS, K.C.B., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

Sir James Stirling, a member of His Majesty's Most Honourable Privy Council, was balloted for and elected a Fellow of the Society.

The following Papers were read:—

I. "On Skin Currents. Part III.—The Human Skin." By Dr. A. D. WALLER, F.R.S.

II. "Antarctic Origin of the Tribe Schœneæ." By C. B. CLARKE, F.R.S.

III. "A New Interpretation of the Gastric Organs of Spirula, Nautilus, and the Gastropods." By J. E. S. MOORE and W. B. RANDLES. Communicated by Professor HOWES, F.R.S.

IV. "Absolute Magnetic Observations at the Valencia Observatory (Cahirciveen, co. Kerry), 1899, 1900, and 1901." By J. E. CULLUM. Communicated by the EARL OF ROSSE, F.R.S.

May 1, 1902.

Sir WILLIAM HUGGINS, K.C.B., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of the Candidates recommended for election into the Society were read, as follows:—

Baker, H. Brereton.
Bovey, Professor Henry T.
Boyce, Professor Rubert.
Brown, John.
Hardy, William Bate.
Harker, Alfred.
Hough, Sidney S.
Kidston, Robert.
Mather, Thomas.
Michell, John Henry.
Newall, Hugh Frank.
Petrie, Professor W. M. Flinders.
Pope, William Jackson.
Saunders, Edward.
Willey, Dr. Arthur.

The following Papers were read:—

I. "Coefficients of the Cubical Expansion of Ice, Hydrated Salts, Solid Carbonic Acid, and other Substances at Low Temperatures." By Professor JAMES DEWAR, F.R.S.

II. "The Conditions determinative of Chemical Change and of Electrical Conduction in Gases, and of the Phenomena of Luminosity." By Professor H. E. ARMSTRONG, F.R.S.

III. "Contributions to a Theory of the Capillary Electrometer."—I. The Insulation-Resistance of the Capillary Electrometer, and the Minimum Quantity of Electricity required to produce a Visible Excursion." By G. J. BURCH, F.R.S.

IV. "The Development of *Echinus esculentus*, together with some Points in the Development of *E. miliaris* and *E. acutus*." By Professor E. W. MACBRIDE. Communicated by A. SEDGWICK, F.R.S.*

* The short paper read November 28, 1901, and printed p. 268 of preceding volume, is to be considered as an abstract of this paper.

May 15, 1902.

Professor H. E. ARMSTRONG, Vice-President, in the Chair.

Sir George D. Taubman Goldie, a member of His Majesty's Most Honourable Privy Council, was balloted for and elected a Fellow of the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Cyanogenesis in Plants. Part II.– The Great Millet (*Sorghum vulgare*)." By Professor W. R. DUNSTAN, F.R.S., and Dr. T. A. HENRY.

II. "On some Phenomena affecting the Transmission of Electric Waves over the Surface of the Sea and Earth." By Captain H. B. JACKSON, R.N., F.R.S.

III. "Microscopic Effects of Stress on Platinum." By T. ANDREWS, F.R.S., and C. R. ANDREWS.

IV. "A Note on the Recrystallisation of Platinum." By W. ROSENHAIN, B.A. Communicated by Professor EWING, F.R.S.

V. "On Electromotive Wave accompanying Mechanical Disturbance in Metal immersed in Electrolyte." By Professor J. C. BOSE. Communicated by Professor REINOLD, F.R.S.

VI. "On the Effect of a Longitudinal Magnetic Field on the Internal Viscosity of Wires of Nickel and Iron, as shown by Change of the Rate Subsidence of Torsional Oscillations." By Professor ANDREW GRAY, F.R.S., and ALEXANDER WOOD.

The Society adjourned over the Whitsuntide Recess to Thursday, May 29.

May 29, 1902.

Sir WILLIAM HUGGINS, K.C.B., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "The Minute Structure of Metals and other Plastic Solids." By G. BEILBY. Communicated by Sir W. C. ROBERTS-AUSTEN, K.C.B., F.R.S.

II. "The Influence of Varying Amounts of Carbon Dioxide in the Air on the Photosynthetic Process of Leaves and on the Mode of Growth of Plants." By H. T. BROWN, F.R.S., and F. ESCOMBE, B.Sc., F.L.S.

III. "On the Influence of an Excess of Carbon Dioxide in the Air on the Form and Internal Structure of Plants." By Professor J. B. FARMER, F.R.S., and S. E. CHANDLER.

IV. "On the Structure of the Gills of Lamellibranchia." By Dr. W. G. RIDEWOOD. Communicated by Professor E. RAY LANKESTER, F.R.S.

June 5, 1902.

Annual Meeting for the Election of Fellows.

Sir WILLIAM HUGGINS, K.C.B., D.C.L., President, in the Chair.

The Statutes relating to the Election of Fellows having been read, Professor Everett and Dr. Buchan were, with the consent of the Society, nominated Scrutators, to assist the Secretaries in the examination of the balloting lists.

The votes of the Fellows present were collected, and the following Candidates were declared duly elected into the Society :—

Baker, H. Brereton.
Bovey, Professor Henry T.
Boyce, Professor Rubert.
Brown, John.
Hardy, William Bate.
Harker, Alfred.
Hough, Sydney S.
Kidston, Robert.
Mather, Thomas.
Michell, John Henry.
Newall, Hugh Frank.
Petrie, Professor W. M. Flinders.
Pope, William Jackson.
Saunders, Edward.
Willey, Dr. Arthur.

Thanks were given to the Scrutators.

June 5, 1902.

Sir WILLIAM HUGGINS, K.C.B., D.C.L., President, in the Chair.

The Right. Hon. Sir George Taubman Goldie, a member of His Majesty's Most Honourable Privy Council, was admitted into the Society.

Major A. W. Alcock (elected 1901) was admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On the Movements of the Flame in the Explosion of Gases." By Professor H. B. Dixon, F.R.S.

II. "Contributions to the Study of Flicker. Paper II." By T. C. Porter. Communicated by Lord Rayleigh, F.R.S.

III. "Effects of Strain on the Crystalline Structure of Lead." By J. C. W. HUMFREY, B.Sc. Communicated by Professor EWING, F.R.S.

IV. "The Spectra of Potassium, Rubidium, and Cæsium, and their Mutual Relations." By H. RAMAGE, B.A. Communicated by Professor LIVEING, F.R.S.

V. "On some Definite Integrals, and a New Method of reducing a Function of Spherical Co-ordinates to a Series of Spherical Harmonics." By Professor A. SCHUSTER, F.R.S.

June 12, 1902.

Professor EMERSON REYNOLDS, Vice-President, in the Chair.

Professor Rubert Boyce, Mr. Thomas Mather, Professor W. M. Flinders Petrie, Mr. William Jackson Pope, and Mr. Edward Saunders were admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "The Influence of an Atmosphere of Oxygen on the Respiratory Exchange." By L. HILL, F.R.S., and J. J. R. MACLEOD.

II. "The Influence of High Pressures of Oxygen on the Circulation of the Blood." By L. HILL, F.R.S., and J. J. R. MACLEOD.

III. "On the Parasitism of *Pseudomonas destructans*." By Professor M. C. POTTER. Communicated by Sir M. FOSTER, Sec. R.S.

IV. "On the Toxic Properties of the Saliva of certain 'Non-poisonous' Colubrines." By Professor A. ALCOCK, F.R.S., and Dr. L. ROGERS.

V. "The Dissipation of Energy by Electric Currents induced in an Iron Cylinder when rotated in a Magnetic Field." By Professor E. WILSON. Communicated by Sir W. PREECE, K.C.B., F.R.S.

VI. "A Note on the Effect of Daylight upon the Propagation of Electromagnetic Impulses over Long Distances." By G. MARCONI. Communicated by Professor FLEMING, F.R.S.

VII. "Note on a Magnetic Detector of Electric Waves which can be employed as a Receiver for Space Telegraphy." By G. MARCONI. Communicated by Professor FLEMING, F.R.S.

June 19, 1902.

Sir WILLIAM HUGGINS, K.C.B., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Correlation between the Barometric Height at Stations on the Eastern Side of the Atlantic." By Miss F. E. CAVE-BROWNE-CAVE and Professor KARL PEARSON, F.R.S.

II. "On Methods for the Limitation and Regulation of Chloroform when administered as an Anæsthetic." By A. VERNON HARCOURT, F.R.S.

III. "On some Phenomena which suggest a Short Period of Solar and Meteorological Changes." By Sir NORMAN LOCKYER, F.R.S., and Dr. W. J. S. LOCKYER.

IV. "Note on the Effect of Mercury Vapour on the Spectrum of Helium." By Professor J. NORMAN COLLIE, F.R.S.

V. "On the Measurement of Temperature. Part I. On the Pressure Coefficients of Hydrogen and Helium at Constant Volume, and at Different Initial Pressures. Part II. On the Vapour Pressures of Liquid Oxygen at Temperatures below its Boiling Point on the Constant Volume Hydrogen and Helium Scales. Part III. On the Vapour Pressures of Liquid Hydrogen at Temperatures below its Boiling Point on the Constant Volume Hydrogen and Helium Scales." By Dr. M. W. TRAVERS, G. SENTER, B.Sc., and Dr. A. JAQUEROD. Communicated by Professor W. RAMSAY, F.R.S.

VI. "The Seed-fungus of *Lolium Temulentum*, L., the Darnel." By E. M. FREEMAN. Communicated by Professor MARSHALL WARD, F.R.S.

VII. "The Stability of the pear-shaped Figure of Equilibrium of a Rotating Mass of Liquid." By Professor G. H. DARWIN, F.R.S.

VIII. "Note on the Conchoidal Fracture of Glass." By W. ROSENHAIN. Communicated by Professor EWING, F.R.S.

IX. "Influence of Temperature on the Conductivity of Electrolytic Solutions." By W. R. BOUSFIELD and Dr. T. MARTIN LOWRY. Communicated by Professor ARMSTRONG, F.R.S.

X. "On an Approximate Solution for the Bending of a Beam of Rectangular Cross-section under any System of Load, with Special Reference to Points of Concentrated and Discontinuous Loading." By L. N. G. FILON. Communicated by Dr. C. CHREE, F.R.S.

XI. "The Geological Structure of Monzoni and the Upper Fassa." By MARIA OGILVIE GORDON. Communicated by Professor LAPWORTH, F.R.S.

The Society adjourned over the Long Vacation to Thursday, November 20.

MINUTES OF MEETINGS

OF

THE ROYAL SOCIETY.

March 6, 1902.

Sir WILLIAM HUGGINS, K.C.B., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of Candidates for election into the Society were read as follows:—

Adeney, Walter Ernest, D.Sc.
Allen, Alfred Henry, F.C.S.
Ardagh, Sir John, Major-General, R.E.
Baker, H. Brereton, M.A.
Ballance, Charles Alfred, F.R.C.S.
Bather, Francis Arthur, M.A.
Bayliss, William Maddock, M.A.
Binnie, Sir Alexander Richardson, M.I.C.E.
Bourne, Gilbert C., M.A.
Bovey, Professor Henry T., M.A.
Boyce, Professor Rubert, M.B.
Bridge, Professor Thomas William, M.A.
Brodie, Thomas Gregor, M.D.
Brown, Professor Adrian John, F.C.S.
Brown, John.
Bruce, John Mitchell, M.D.
Budge, Ernest A. Wallis, Litt.D.
Callaway, Charles, D.Sc.
Cardew, Philip, Major R.E.
Chattaway, Frederick Daniel, M.A.
Clowes, Frank, D.Sc.
Copeman, Sydney Monckton, M.D.
Corfield, Professor William Henry, M.D.
Crookshank, Professor Edgar March, M.B.
Darwin, Horace, M.A.
Davison, Charles, Sc.D.
Dendy, Professor Arthur, D.Sc.
Dobbie, Professor James Johnstone, M.A.
Goodrich, Edwin S., M.A.
Gray, Professor Thomas, B.Sc.
Harcourt, Prof. Leveson Francis Vernon, M.A.
Hardy, William Bate, M.A.

Harker, Alfred, M.A.
Harmer, Frederic William, F.G.S.
Hiern, William Philip, M.A.
Hills, Edmond Herbert, Major R.E.
Hopkinson, Edward, D.Sc.
Hough, Sydney Samuel, F.R.A.S.
Jukes-Browne, Alfred John, F.G.S.
Kidston, Robert, F.G.S.
Knott, Cargill Gilston, D.Sc.
Lees, Charles H., D.Sc.
Letts, Professor Edmund Albert, D.Sc.
Lewis, Sir William Thomas, Bart., M.Inst.C.E.
MacArthur, John Stewart, F.C.S.
Maclean, Prof. Magnus, D.Sc.
MacMunn, Charles Alexander, M.D.
Mallock, Henry Reginald Arnulph, M.Inst.C.E.
Mance, Sir Henry C., C.I.E.
Masson, Professor Orme, M.A.
Mather, Thomas.
Matthey, Edward, F.C.S.
Maunder, Edward Walter, F.R.A.S.
Meyrick, Edward, B.A.
Michell, John Henry, M.A.
Mill, Hugh Robert, D.Sc.
Molesworth, Sir Guilford, K.C.I.E.
Newall, Hugh Frank, M.A.
Notter, James Lane, Surg. Lieut.-Col., M.D.
Nuttall, George Henry Falkner, M.D.
Oliver, John Ryder, Major-General (late R.A.), C.M.G.
Parsons, Professor Frederick Gymer, F.R.C.S.
Payne, Joseph Frank, M.D.
Perkin, Arthur George, F.C.S.
Petrie, Prof. William Matthew Flinders, D.C.L.
Plimmer, Henry G., M.R.C.S.
Pope, William Jackson, F.C.S.
Rose, Thomas Kirke, D.Sc.
Russell, James Samuel Risien, M.D.
Rutherford, Professor Ernest, M.A.
Salomons, Sir David, Bart., M.A.
Saunders, Edward.
Sclater, William Lutley, M.A.
Sharpe, R. Bowdler, LL.D.
Sidgreaves, Rev. Walter, S.J., F.R.A.S.
Smith, Fred., Lieut.-Col.
Smith, James Lorrain, M.D.
Smith, William Robert, M.D.
Stead, John Edward, F.C.S.
Strahan, Aubrey, M.A.
Swinburne, James, M.Inst.C.E.
Swinton, Alan Archibald Campbell, Assoc. M.Inst.C.E.
Symington, Prof. Johnson, M.D.
Tarleton, Professor Francis Alexander, Sc.D.
Tatham, John F. W., F.R.C.P.
Townsend, Professor John S., M.A.
Wager, Harold, F.L.S.
Walker, James, M.A.
Watkin, Colonel, H. S. S., R.A., C.B.
White, William Hale, M.D.
Whitehead, Alfred North, M.A.
Willey, Arthur, D.Sc.
Wilson, Professor Ernest.
Woodhead, Professor German Sims, M.D.

The following Papers were read:—

I. "On the Spark Discharge from Metallic Poles in Water." By Sir NORMAN LOCKYER, K.C.B., F.R.S.

II. "Experimental Researches on Drawn Steel. Part I.—Magnetism and Changes of Temperature. Part II.—Resistivity, Elasticity, and Density, and the Temperature Coefficients of Resistivity and Elasticity." By J. R. ASHWORTH. Communicated by Professor SCHUSTER, F.R.S.

III. "On the Effect of Magnetisation on the Electric Conductivity of Iron and Nickel." By G. BARLOW. Communicated by Professor A. GRAY, F.R.S.

IV. "The Differential Equations of Fresnel's Polarisation-Vector, with an Extension to the Case of Active Media." By JAMES WALKER. Communicated by Professor R. B. CLIFTON, F.R.S.

V. "On a Convenient Terminology for the Various Stages of the Malaria Parasite." By Professor E. RAY LANKESTER, F.R.S.

March 13, 1902.

Sir WILLIAM HUGGINS, K.C.B., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The Croonian Lecture, "On Certain Chemical and Physical Properties of Hæmoglobin," was delivered by Professor A. GAMGEE, F.R.S.

March 20, 1902.

Sir WILLIAM HUGGINS, K.C.B., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Development of the Layers of the Retina in the Chick after the Formation of the Optic Cup." By JOHN CAMERON, M.B., Ch.B. (Edin.). Communicated by Professor MCINTOSH, F.R.S.

II. "On a Peculiarity of the Cerebral Commissures in certain Marsupialia, not hitherto recognised as a Distinctive Feature of the Diprotodontia." By Professor G. ELLIOT SMITH, M.D., Ch.M. Communicated by Professor HOWES, F.R.S.

III. "The Classification of the Elements." By Professor H. E. ARMSTRONG, V.P.R.S.

IV. "Persulphuric Acids." By Professor H. E. ARMSTRONG, V.P.R.S., and Dr. T. M. LOWRY.

V. "On a Throw-testing Machine for Reversals of Mean Stress." By Professor OSBORNE REYNOLDS, F.R.S., and J. H. SMITH, M.Sc.

VI. "On the Equilibrium of Rotating Liquid Cylinders." By J. H. JEANS, B.A. Communicated by Professor G. H. DARWIN, F.R.S.

VII. "A Portable Telemeter or Range-finder." By Professor GEORGE FORBES, F.R.S.

The Society adjourned over the Easter Recess to Thursday, April 24.

PROCEEDINGS

OF

THE ROYAL SOCIETY.

"A Comparative Study of the Spectra, Densities, and Melting Points of some Groups of Elements, and of the Relation of Properties to Atomic Mass." By HUGH RAMAGE, B.A., A.R.C.Sc.I., St. John's College, Cambridge. Communicated by Professor G. D. LIVEING, F.R.S. Received November 7,—Read November 28, 1901.

[PLATES 1, 2.]

Introduction.

This investigation was begun in the hope that it would lead to the discovery of some of the laws which determine the distribution of lines and bands in spectra: those laws, more especially, which govern the changes of oscillation frequency of corresponding lines in the spectra of some of the metals.

A. Mitscherlich appears to have been the first to make a comparative study of spectra.* He compared the spectra of the haloid salts of the metals of the alkaline earths. He found that individual lines recur in the spectra of one and the same metal, which, according to the halogens, are more or less distant from one another. Fluorides form an exception. In the spectra of barium compounds the distances (on the scale of his spectroscope) between two prominent lines in the various spectra were to each other as the "atomic weights" of the compounds. In barium chloride the distance between the two lines was 3·9 scale divisions. If x is the difference between the two corresponding lines in the spectrum of barium iodide, then:—

$$\frac{3\cdot9}{x} = \frac{104}{195\cdot5} = \frac{BaCl_2}{BaI_2}.$$

Mitscherlich determined a common starting point for the haloid salts of barium, and extended the work to the spectra of compounds of calcium and strontium.

* 'Pogg. Ann.,' vol. 121, pp. 459—488, 1864; 'Phil. Mag.,' vol. 28, p. 169, 1864.

Lecoq de Boisbaudran,* in proposing a theory on the origin of the spectral lines, made the following observations :—

"Les raies spectrales des métaux alcalins (et alcalinoterreux), classés par leur réfrangibilités, sont placées, comme les propriétés chimiques, suivant l'ordre des poids atomiques."

"Le spectre du rubidium paraît donc alors analogue à celui du potassium, étant seulement transporté, comme tout d'une pièce, vers le rouge."

Boisbaudran later (1870) laid stress upon these analogies, especially between the spectra of potassium, rubidium and cæsium, but he made no reference to the spectra of lithium and sodium.

In 1886† he extended his comparative researches on spectra, and calculated the atomic weights of gallium and germanium from data supplied by their spectra and the atomic weights and spectra of other elements. Ames criticised this work,‡ and showed that the method failed when applied to other elements.

The discovery of harmonic series of lines in the spectra of elements by Liveing and Dewar,§ and the work of Hartley|| on "Homologous spectra," indicated that there was a general law which applied to the production of the spectra of several elements. The formulæ which have been applied to these series, notably by Balmer, Kayser and Runge, and, more especially, the formula and work of Rydberg, have emphasised this fact in a most striking manner. Rydberg¶ discussed the spectra of the more common monad, dyad and triad metals very fully, and enunciated several important laws. His work will be considered in some detail in the course of this paper.

Methods available for the Selection of the Corresponding Lines in Spectra.

Ames, in discussing the difficulty of discovering these lines,** said that the first really scientific work in this direction was done by Hartley's discovery of the constant differences of oscillation frequency between the members of doublets and triplets.††

Liveing and Dewar's harmonic series of lines, and the formulæ of Rydberg and of Kayser and Runge, give valuable assistance in this part of the work.

The author was led to *begin* this investigation by the knowledge of flame spectra which he acquired whilst working with Professor Hartley.

* 'Comptes Rendus,' vol. 69, 1869.

† *Ibid.*, vol. 102, pp. 1291–5.

‡ 'Phil. Mag.,' 5th series, vol. 30, p. 47, 1890.

§ 'Phil. Trans.,' vol. 174, pp. 187–222, 1883.

|| 'Trans. Chem. Soc.,' vol. 43, p. 390, 1883.

¶ 'Kongl. Svenska Vetensk. Akad. Handl.,' vol. 23, No. 11, 1890.

** 'Phil. Mag.,' vol. 30, 1890, p. 47.

†† 'Trans. Chem. Soc.,' vol. 43, p. 390–400.

Upwards of thirty metals yield spectra when heated in the oxyhydrogen flame: these metals also yield arc and spark spectra. The flame spectra are the simplest: the spark spectra are the most complicated.

Under the conditions which obtain in flames, the metals are usually heated to a temperature not very far removed from their boiling points, while the electrical conditions are probably simpler than obtain in the electric arc or spark. There are present in the Bunsen flame, in addition to the vapour of the metal, various gases such as: hydrogen, oxygen, oxides of carbon, steam and nitrogen, all heated to a high temperature. Some of the chemical changes which take place in the flame are reversible, and the metallic vapours are consequently in presence of gases, some of which are combining together, some are the compounds formed, and some of the latter are undergoing dissociation. Similar changes occur in the oxycoal-gas flame. The reactions in the oxyhydrogen flame are simpler, but similar spectra of metals are obtained from both of these high temperature flames.

It is possible, by varying the temperature of the flame, to obtain the simplest or fundamental spectra of many of the metals, and also to study the order of appearance of additional lines as more complex spectra are produced.

The metals of the alkalies and alkaline earths give spectra in the Bunsen flame. The former yield line spectra, the latter yield spectra composed of lines and bands. The bands and some of the lines have been attributed to the oxides of the metals. Indium gives a spectrum composed of two lines, and thallium a spectrum of one green line in the Bunsen flame. No other metals give spectra of importance in this flame.

Spark spectra of salt solutions, produced without the aid of a Leyden jar, are very similar to the arc and high temperature flame spectra of the metals present in those salts.

Flame spectra, then, furnish purely experimental data with which to begin an investigation of the laws which govern the distribution of the lines in spectra, and by which to study the relations of the physical and chemical properties of the metals to their spectra. Further experimental data are furnished in studying more complex spectra by:—

(*a.*) The self-reversal of lines.

(*b.*) The character of the lines—sharp or nebulous; continuous or discontinuous.

(*c.*) Hartley's discovery of the constant differences between the oscillation frequencies of the components of doublets and triplets.

(*d.*) The Zeeman effect.

(*e.*) The effect on the lines of a change of atmosphere surrounding the source of light.*

(*f.*) Shift of lines under changes of pressure.†

(*g.*) Harmonic series of lines.

The formulæ of Rydberg and of Kayser and Runge should be used in connection with the last of the above.

The experimental data are still incomplete, especially in the red, infra-red and ultra-violet regions. We now believe that the greatest accuracy of measurement and the fullest details of the ultra-violet lines are most necessary for the completion of this investigation. As far as can be seen from the data now available, all the above methods lead, and will lead, to the selection of the same lines as being produced by corresponding motions of or in the molecules. Much experimental work must yet be done, and it is hoped that the present investigation will indicate the direction and character of the work most needed.

Flame Spectra.

Bunsen Flame Spectra.—Eder and Valenta have studied these over the greatest range of spectrum. They, by heating the chlorides of lithium, sodium, potassium, calcium, strontium, and barium in the Bunsen flame, obtained spectra composed of lines and bands, which they attributed to the metal, oxide, or chloride in each spectrum.‡

Oxyhydrogen Flame Spectra.—Hartley has studied these in the ultra-violet region and in the more refrangible portion of the visible spectrum.§ The author assisted Professor Hartley in this work, and from the summer of 1893 until 1899 the investigation was conducted as a joint work. The spectra of nearly all the elements were photographed in the course of the investigation: some of them gave only continuous spectra, and these cannot be included in the present work. Of the others, the following nineteen have been selected and studied in detail:—

(1.) Lithium, sodium, potassium, rubidium, and cæsium.
(2.) Copper, silver, and gold.
(3.) Magnesium, zinc, cadmium, mercury.
(4.) Calcium, strontium, barium.
(5.) Aluminium, gallium, indium, thallium.

The following metals also yield line flame-spectra, but the spectra have not been grouped as perfectly, nor studied as fully, as those of

* Crew, 'Phil. Mag.,' November, 1900.
† Humphreys and Mohler, 'Astrophys. Jour.,' vol. 3, p. 114, 1896.
‡ 'Sitzber. Kais. Akad. Wien,' vol. 60, 1893.
§ 'Phil. Trans.,' A, 1894, 161–212.

the metals given above:—Tin, lead, bismuth, chromium, manganese, iron, cobalt, nickel, ruthenium, rhodium, palladium, and iridium.

The above nineteen metals may be* classified by their spectra, as shown, into groups in agreement with Mendeléeff's Periodic Law. The spectra of the metals in each group are similar to one another, and they are quite different from the spectra of the other groups.

The Diagrams.

It is apparent, when comparing the spectra of each group, that the positions of the strongest lines, and of the others in order, change regularly with the increase of atomic mass of the metals. The change in position is apparently so simple that it suggested a graphical method of representing the spectral lines as functions of the atomic mass. The lines were plotted as abscissæ, and the atomic masses as ordinates. Two diagrams were drawn at first, one from the oscillation frequencies of the lines, and the other from the wave-lengths. Connecting lines were then drawn through the corresponding lines in homologous spectra. These were mostly curved, and it was thought that the equations to these curves might be discovered by further study, and also that some relation might possibly be discovered between the equations.

Several curious results were obtained by observations on the points in which the converging lines, drawn through the corresponding members of doublets and triplets, intersected. These points were on the same horizontal line in each group, but it was difficult to determine their position accurately, and the results have since been regarded more as coincidences. The method, however, gave promise of yielding valuable results, and the research has been continued, partly by a further investigation of flame spectra and partly by an improvement in the method of drawing the diagrams. Use has been made also of some of the work of Rydberg, Preston, and others, to whom reference will be made in the paper, to make the research as accurate and complete as possible.

Discussion of the Spectra and the Lines.

It may be stated at once that nearly all the lines observed in the flame spectra of the metals considered have been included in the diagrams. A few lines, selected from arc and spark spectra, have been used, but only in cases where our knowledge of flame spectra is incomplete.

There is no difficulty in selecting the corresponding lines from some spectra.

Calcium, Strontium, Barium.—The oxyhydrogen flame spectrum of each

* Hartley and Ramage, 'Trans. Roy. Dublin Soc.,' N.S., vol. 7, pt. 12.

of these contains one line which is very much stronger than any of the other lines. The wave-lengths are:—Calcium 4227, strontium 4607, barium 5536. Eder and Valenta record them also in Bunsen flame spectra of the chlorides of these metals. They attribute the following lines to the *metals*:—

	Intensity in—		
	Bunsen.	Oxyhydrogen.	Arc.
Calcium—			
4227.....	10	10	10*r*
Strontium—			
5968.....	2	—	—
4608.....	10	10	10*r*
4032.....	2	—	4*br*
Barium—			
6497.....	2	7	6*r*
5536.....	10	10	10*r*

It is very doubtful if the strontium lines, wave-lengths 5968 and 4032, are due to the metal. They should, if metallic lines, appear in both flame and arc spectra. The barium line λ 6497 is probably due to the metal, as also is a line λ "4554 (?)," which Eder and Valenta attribute to the oxide of barium: it is present both in the oxyhydrogen and arc spectra.

The three lines in question are practically the only lines due to the metals in their Bunsen flame spectra: they are the strongest lines in the oxyhydrogen flame spectra, and they are reversed in the arc spectra. No other line has been observed which resembles them, so we conclude that they correspond to one another: that they are produced by similar motions of the molecules.

These lines are probably the only representatives of the principal series of these elements.

Rydberg also selects these three lines as corresponding to one another.

There are other lines in the oxyhydrogen spectra of these elements which must be considered. These present difficulties, and it is probable that it will be necessary to examine them under the "Zeeman effect," before a final selection can be made. Some of the lines of barium which should correspond to those observed in calcium and strontium will probably be found in the red region of the spectrum. Ten lines have been observed on the less refrangible side of the line λ 4227 in the spectrum of calcium; eleven have been observed in strontium on the less refrangible side of line λ 4607, but none have

yet been observed corresponding to these in the spectrum of barium. On the more refrangible side of these three lines, two lines, corresponding to the solar lines H and K, wave-lengths 3968·8 and 3933·8, are found in calcium; two lines, wave lengths 4215·7 and 4077·9, in strontium, and seven lines in barium. The two calcium lines doubtless correspond to the two strontium lines, but it is very difficult to select the corresponding lines of barium. The two selected, and included on the diagrams, have wave-lengths 3890 and 3500, but they are the only lines about which serious doubt is held. We must not therefore attach much importance to them until the spectrum of barium has been fully examined and the selection confirmed.

Humphreys and Mohler* found that the H and K lines of calcium were only shifted half as far as the 4226·9 line by changes of pressure.

Reese† has shown that under the Zeeman effect the 3933 line of calcium becomes a triplet and the 3968 line a quadruplet.

Lithium and Sodium.—There is no doubt but that the red lithium line corresponds to the yellow (D) lines of sodium. These lines are the strongest in the spectra, and there are no other lines of the principal series of these two elements nearer than wave-length 3303.

The two subordinate series may be easily arranged, according to the intensity of the lines: the lines of the diffuse series are much stronger than those of the sharp series.

Potassium, Rubidium, and Cæsium.—Lines which, from their relative intensities in all spectra, appear to correspond to one another are :—

	Intensity.		
	Bunsen.	Oxyhydrogen.	Arc.
Potassium—			
4047.....	} 10 {	8	6*r*
4044.....		10	8*r*
Rubidium—			
4216.....	9	8	6*r*
4202.....	10	10	8*r*
Cæsium—			
4593.....	9	6	6*r*
4555.....	10	10	8*r*

These are all given by the same value of m (= 2) in Rydberg's formula for the principal series, and the other lines of the two series follow in order. The lines in the diffuse subordinate series were

* 'Astrophys. Jour.,' vol. 3, 1896 p. 114.

† *Ibid.*, September, 1900.

selected in the same way, but our knowledge of these series is not as complete as our knowledge of the principal series. Kayser and Runge did not observe any lines in the sharp subordinate series of cæsium: no lines, therefore, of the sharp series appear on the diagrams.

Copper, Silver, Gold.—There are only two strong lines in the flame spectra of copper and silver, but some weak lines occur in the less refrangible region. The wave-lengths of the strong lines are:—Copper, 3274·1 and 3247·6; silver, 3380·8 and 3283·0.

A line with wave-length 2675 occurs in the oxyhydrogen flame spectrum of gold. It is much more refrangible, therefore, than the above lines of copper and silver, and is on this account much weaker; the same* source of energy does not produce these more rapid motions with the same amplitude as the slower motions. This line and another of wave-length 2428 occur in the arc and spark spectra of gold: the two lines are easily reversed, and appear in all respects to correspond to the above doublets of copper and silver. Rydberg selected these six lines as the first members of the principal series of these metals. He gave the wave-lengths, on Ångström's scale, as:—

	Copper.	Silver.	Gold.
P_2	3273·2	3382·3	2675·4
P_1	3246·9	3280·1	2427·5

There are also several remarkable† bands in the flame spectra of these three metals. The bands in the silver spectrum differ in character from those in the copper and gold spectra. Every band terminates in a sharp line on the more refrangible side. The lines forming the bands are sharper and more widely separated in the spectra of copper and gold than in the spectrum of silver.

Magnesium, Zinc, Cadmium, Mercury.—The flame spectra of the first three consist of lines and bands. The bands are very complicated in structure, and are degraded on the more refrangible side. Eder and Valenta‡ and Huff§ have described the production of a banded spark spectrum of mercury, which is similar in character to those of zinc and cadmium. No bands degraded in the same direction have been discovered in the spectra of other metals.

The following lines occur in the flame spectra of these metals:—

* Hartley and Ramage, 'Trans. Roy. Dublin Soc.,' N.S., vol. 7, part xii, p. 341.

† Hartley and Ramage, *ibid.*

‡ 'Denkschr. K. Akad. Wien,' vol. 61, 1894.

§ 'Astrophys. Jour.,' Sept. 1900.

Magnesium.	Zinc.	Cadmium.	Mercury.
5184 } triplet 5173 } 5168 } 4571 2852	4812 } triplet 4722 } 4680 } 3076	5086 } triplet 4800 } 4678 } 3261	547 435

Mercury appears to give only a continuous spectrum in the oxyhydrogen flame. Both zinc and cadmium give weak spectra, proving that the emissive power of their molecules in the oxyhydrogen flame is low compared with many other metals. Mitscherlich* records two lines in the spectrum of cyanide of mercury, heated in the oxyhydrogen or oxycoal-gas flame, which correspond to two lines of the strongest triplet in the arc and spark spectra of mercury. The wave-lengths of the lines of the triplet are 5460, 4359, and 4078. This triplet is given by the merest trace of mercury in a vacuum tube.

There is every reason for believing that the four triplets correspond to one another. The lines are all given by the same value of m in Rydberg's formula. The work of Preston,† of Lord Blythswood and Dr. Marchant,‡ and of Reese§ on the Zeeman effect give additional support to this view.

Observer.	Magnesium, zinc, and cadmium.		Mercury.		Mercury.
	Preston.		Blythswood and Marchant.		Reese.
Strength of magnetic field. C.G.S. units.	20,000	40,000	6,000	24,000	24,500
Least refrangible line	Nebulous triplet	Triple triplet	Triplet	Doublet, triplet, doublet	‖Triplet, "probably more."
Middle line	Quartet	Sextet	Quartet	Sextet	Triplet.
Most refrangible line	Triplet	Triplet	—	—	Triplet.

There remain the isolated lines: two of magnesium and one each of zinc and cadmium.

* 'Phil. Mag.,' Sept. 1864, plate.

† 'Trans. Roy. Dublin Soc.,' vol. 7, series ii, No. 2.

‡ 'Phil. Mag.,' vol. 49, p. 384, 1900.

§ 'Astrophys. Jour.,' Sept. 1900, p. 120.

‖ Gray has now resolved this into nine lines, 'Brit. Assoc.,' 1901.

The four lines occur in the arc spectra of their respective elements: the zinc line is in Kayser's and Runge's tables, marked 8*r*; the magnesium line 4574 is marked 4, the remaining two 10*r*. These characters indicate that the magnesium line λ 2852 corresponds to the lines of zinc and cadmium. The magnesium line λ 2852 has not been examined under the Zeeman effect, but the zinc line breaks up into a "very sharp triplet" and the cadmium line into a "triplet" (Reese). The magnesium line is given when a very small quantity of the element is heated in the oxyhydrogen flame, or by a very short exposure; it furnishes, in fact, a delicate test for the element. All this evidence indicates that the line λ 2852 corresponds to the zinc and cadmium lines.

Rydberg says (p. 106) of the magnesium line λ 2852: "It is probable. . . . (it) corresponds to the strongest lines in the spectra of the elements:"—Calcium 4226, strontium 4607, and barium 5534. The evidence furnished by the oxyhydrogen flame spectra is against this conclusion, for it shows no similarity between the spectrum of magnesium and those of the calcium group. The bands in the flame spectra of the calcium group are degraded on the less refrangible side, and they are quite different in character from those of the zinc group.

Rydberg says (p. 114) that the lines of zinc 3075·6 and cadmium 3260·12 "correspond perfectly." He does not refer to a corresponding line in the spectrum of mercury. There is one, doubtless, and its discovery will be of considerable interest.

Ames* selected the following lines from the arc spectra. Magnesium 2852·2, zinc 2138·3, and cadmium 2288·1. These lines of zinc and cadmium have not been observed in the oxyhydrogen spectra. Even if present they must, on account of their high refrangibility, be very weak. The zinc line λ 3076, lies, in the flame spectrum, among the lines in the strongest water vapour group, and it was photographed for the first time, in a special search for it, in May, 1900.

The Aluminium Group.—All the lines found in the flame spectra of the metals of this group are used in the investigation. The selection of the corresponding lines presents no difficulty, as the corresponding lines are clearly indicated by the relative intensity of the lines, and by the fact that the less refrangible member of one series of doublets is itself double. Five lines have been observed in the flame spectrum of gallium, but this spectrum has not been fully examined. The wave-lengths of the other five lines were determined on two photographs of the spark spectrum of gallium which Professor Liveing very kindly lent me.

Exner and Haschek have measured some lines in the spark spectrum of gallium.†

* *Loc. cit.*

† 'Akad. Wiss. Wien, Sitzb.,' 108, p. 1120, 1899.

Reese has shown that the 3944 line of aluminium yields, under the Zeeman effect, a quadruplet and the 3961 line a triplet.

If we confine our attention to the lines of the flame spectra of the metals the diagrams are comparatively simple. The work may be extended, however, by including the more refrangible lines in the arc and spark spectra of several metals. Rydberg's formula, and the formula of Kayser and Runge, will assist greatly in this extension of the work.

The study of the effect of a magnetic field on the source of light (the "Zeeman Effect") will be of great importance in confirming the selection of a few of the lines included in the present paper, and in the prosecution of the investigation in the future.

The more refrangible lines are always feeble and nebulous compared with the less refrangible members of the series, and it is more difficult to obtain accurate measurements of these weaker lines. The principal series of lithium furnishes us with a typical example of this difficulty. The lines were measured in the arc spectrum by Kayser and Runge.

Wave-lengths.	Limit of error.	Intensity. 10 = maximum.
6708·2	0·2	10
3232·77	0·03	8
2741·39	0·03	6
2562·60	0·03	4
2475·13	0·1	4
2425·55	0·1	2
2394·54	0·2	1

If we except the line in the extreme red, the accuracy of the determination of the wave-lengths diminishes with the intensity and refrangibility of the line.

Discussion of the Diagrams (*Plates* 1 *and* 2).

The diagrams were first drawn with the atomic masses as ordinates, and the lines connecting corresponding lines were nearly all curved. Dr. J. H. Vincent, of this college, suggested to me, in February, 1900, that different functions of the atomic masses should be used as ordinates, so that as many of these curved lines as possible should be converted into straight lines. It was found immediately that, by taking the squares of the atomic masses, the line joining the strongest lines of calcium, strontium, and barium—wave-lengths 4227, 4608, and 5536 respectively—was very nearly straight.

Diagrams have since been drawn to this scale, both in oscillation frequencies and in wave-lengths, of all the lines enumerated above.

List of the Corresponding Lines included in the Diagrams.

m.	Wave-length.	Intensities. Bunsen.	Oxy-H.	Arc.	Spark.	*m.*	Wave-length.	Intensities. Bunsen.	Oxy-H.	Arc.	Spark.
	Lithium.						Potassium—*continued.*				
1	6708·2	10	10	10*r*	10						
2	3232·82	4	4	8*r*		6	3034·94	—	—	4*r*	
3	2741·43	—	1	6*r*		7	2992·33	—	—	2*r*	4
4	2562·60	—	—	4*r*		8	2963·36	—	—	1*r*	
5	2475·13	—	—	4*r*		9	2942·8	—	—	1*r*	
6	2425·55	—	—	2*r*							
7	2394·54	—	—	1*r*							
8	2373·9	—	—	<1			Rubidium.				
9	2359·4	—	—	<1							
						1	7950·46	4	9	10*r*	
						1	7805·98	8	10	10*r*	
	Sodium.					2	4215·68	9	9	6*r*	
						2	02·04	10	10	8*r*	
1	5896·16	10	10	10*r*	10	3	3591·86	—	3	4*r*	
1	5890·19	10	10	10*r*	10	3	87·27	—	4	6*r*	
2	3303·07	8	3	8*r*	10	4	3350·98	—	1	2*r*	
2	2·47	—	4	8*r*	10	4	48·84	—	2	4*r*	
3	2853·02	2	2	6*r*	10	5	3229·26	—	1		
4	2680·70	—	1	4*r*	8	5	8·18	—	1		
5	2593·98	—	—	2*r*	3						
6	2543·85	—	—	1*r*	1						
7	2512·23	—	—	1*r*	1		Cæsium.				
						1	8949·92	—	—	9	
	Potassium.					1	8527·72	—	—	10	
						2	4593·30	9	8	6*r*	
1	7699·3	10	10	10*r*		2	55·46	10	10	8*r*	
1	7655·6	10	10	10*r*		3	3888·75	—	2	4*r*	
2	4047·39	10	9	6*r*	10	3	76·31	—	4	6*r*	
2	4044·33		10	8*r*		4	3617·49	—	<1	2*r*	
3	3447·56	4	3	6*r*	10	4	1·70	—	2	4*r*	
3	6·55		4	8*r*		5	3447·25	—	1		
4	3217·76	1	1	4*r*	2	6	3398·40	—	1		
4	7·27		2	6*r*		7	48·72 ?	—	<1		
5	3102·37	—	—	2*r*	1	8	14·0	—	<1		
5	2·15	—	—	4*r*	1	9	3287·0	—	<1		

Wave-length.	Intensities.				Wave-length.	Intensities.			
	Bunsen flame.	Oxy-H.	Arc.	Spark.		Bunsen flame.	Oxy-H.	Arc.	Spark.
Copper.					**Gold.**				
3274·11	—	9	10*r*	8	2676·05	—	3	10*r*	8
3247·68	—	10	10*r*	10	2428·10	—	—	10*r*	10
Silver.									
3383·06	—	10	10*r*	10					
3280·84	—	10	10*r*	10					
Calcium.					**Barium.**				
4226·91	10	10	10*r*	12	5535·69	10	10	10*r*	10
3968·83	—	3	10*r*	10	3889·45	—	2	4	
3933·83	—	4	10*r*	10	3501·25	—	3	10	10
Strontium.									
4607·45	10	10	10	10					
4215·66	—	4	10*r*	10					
4077·88	—	3	10	10					
Magnesium.					**Cadmium.**				
5138·84	—	10	10*r*	10	5086·06	—	10	10*r*	10
5172·87	—	9	10*r*	9	4800·09	—	9	10*r*	10
5167·55	—	8	8*r*	8	4678·37	—	8	10*r*	10
2852·21	—	10	10*r*	10	3261·18	—	10	10*r*	8
Zinc.					**Mercury.**				
4801·71	—	10	10*r*	10	5790·49	—	—	10*r*	10
4722·26	—	9	10*r*	10	5769·45	—	—	10*r*	10
4680·38	—	8	10*r*	10	5460·97	—	—	10*r*	10
3076·02	—	10	8*r*	8					
Aluminium.					**Indium.**				
3961·68	—	10	10*r*	9	4511·44	10	10	10*r*	10
3944·16	—	10	10*r*	9	4101·87	10	9	8*r*	9
3092·95	—	3	6*r*	9	3258·71	—	3	6*r*	9
3092·84			10*r*		6·22	—	4	10*r*	10
3082·27	—	3	10*r*	9	3039·46	—	4	10*r*	10
2660·49	—	—	10*r*	5	2932·71	—	2	6*r*	7
2652·56	—	—	10*r*	5	2753·77	—	1	6*r*	5
2575·49	—	—	6*r*	7	2714·05	—	>1	6*r*	3
2575·20	—	—	10*r*		10·38			10*r*	7
2568·08	—	—	10*r*	7	2560·25	—	—	8*r*	7

Wave-length.	Intensities.				Wave-length.	Intensities.			
	Bunsen flame.	Oxy-H.	Arc.	Spark.		Bunsen flame.	Oxy-H.	Arc.	Spark.
Gallium.					Thallium.				
4172·21	—	10	10	10	5350·65	10	10	10*r*	10
4033·12	—	9	9	9	3775·87	—	10	10*r*	10
2943	—	2	—	3	3529·58	—	3	8*r*	10
2	—	3	—	4	19·39	—	4	10*r*	10
2873	—	3	—	4	2767·97	—	3*r*	10*r*	10
2780	—	—	—	3	3229·88	—	3	10*r*	8
2718	—	—	—	2	2580·23	—	—	8*r*	8
2496 broad	—	—	—	2	2921·63	—	1	6*r*	8
2447	—	—	—	1	18·43	—	3	10*r*	10
					2379·66	—	—	8*r*	8

Diagrams have been drawn with other functions of the atomic mass, but they have not given results of any great value.

The diagrams reproduced in Plates 1 and 2 were drawn from the oscillation frequencies of the lines and (1) the atomic masses, (2) the squares of the atomic masses. The symbols indicate the positions of the lines in the spectra.

The following facts have been observed in the study of the diagrams :—

(1.) The metals considered may be classified into groups, according to their spectra. The fact is not new, but the diagrams make its truth most obvious.

(2.) The connecting lines between the members of the groups are not continuous : there are certain breaks in them. There is a break in the group of the metals of the alkalies between sodium and potassium. Another break occurs between the triplet of magnesium and those of zinc, cadmium, and mercury.

There are members of two subordinate series in the spectra of the aluminium group—the sharp and the nebulous series. The break between aluminium and the other metals is very marked in the diffuse series, while there is only a slight irregularity in the lines connecting the sharp series. This fact will be referred to again when considering the physical properties of the metals.

(3.) The cause of the displacement of corresponding lines in some strictly homologous spectra is intimately connected with the change in the atomic mass. The lines are moved towards the less refrangible end of the spectrum with an increase in the atomic mass. The shift of the lines due to this cause is most

evident in the lines of the metals of the alkalies, particularly of potassium, rubidium, and cæsium, and in the strongest lines of calcium, strontium, and barium.

In Plate 1 the lines joining the diffuse subordinate series of the potassium group approach to straight lines in the more refrangible members of the series, and the lines joining the principal series are nearly parabolic. The latter are nearly straight in the more refrangible members in Plate 2. The shift of the subordinate lines is approximately proportional to the atomic mass, and the shift of the principal lines to the square of the atomic mass.

The fundamental lines of calcium, strontium, and barium are given by the equation $n = 24170 - 0{\cdot}3232\,W^2$, in which $n = 10^8\,\lambda^{-1}$ and W = atomic mass. The differences from the observed values are—calcium 0, strontium -7, and barium $+9$.

The atomic mass is not the only cause which determines the shift of the lines. There are, doubtless, other causes at work, and it is probably in these we must seek the explanation of the abnormal shifts which produce the breaks in the connecting lines referred to above. In passing from group to group, for instance, an increase in the valency with but slight increase in the atomic mass results in the whole of a series being displaced to a considerable extent. The following figures have been taken to illustrate this point from Rydberg's "La Distribution des Raies Spectrales," read to the Congrès International de Physique de 1900:—

Element.	Atomic Mass.	Difference in Atomic Mass.	Valency.	Limit of Series. Oscillation Frequencies.	Difference in limit of series.
Sodium	23·06	1·32	I	24470	15285
Magnesium ..	24·38	2·70	II	39755	8401
Aluminium ..	27·08		III	48156	
Silver	107·94	4·14	I	30648	10069
Cadmium	112·08	1·62	II	40717	3731
Indium	113·7		III	44448	

The figures in the fifth and sixth columns refer to the subordinate series of lines.

Changes of valency therefore produce much greater displacements of the spectral lines than changes in the atomic masses.

Another cause is doubtless at work in the elements—copper, silver, and gold. The connecting lines here take a remarkable form, and this form is seen also in the lines selected as corresponding to the H and K solar lines of calcium in the spectra of calcium, strontium, and barium.

Many of the chemical and physical properties of these elements have been considered, and the only common property yet discovered is connected with the atomic volume.

	Atomic volume.		Atomic volume.
Copper	7·2	Calcium	25
Silver	10	Strontium	35
Gold	10	Barium	36

The atomic volumes of the two elements of higher atomic weights in each group are almost identical.

If it should prove that the two lines of barium do not correspond to the strontium and calcium lines, we may find that the remarkable curves joining the lines of copper, silver, and gold are connected with the valency of these metals. The curve of melting points takes a peculiar form also in these three metals.

(4.) *Intersection of the Lines connecting Homologous Doublets and Triplets.*

Rydberg* denoted by v the difference in oscillation frequencies: (1) between the members of doublets; (2) between the two more widely separated lines in triplets. He made the following observations:—1st. "The value of v increases in special families with increase of atomic weight." 2nd. "The constant v is, without doubt, a very complicated function of the atomic weight."

These observations were made entirely on the subordinate series of lines, and he found that the values of $10^3v/P^2$ (where P = the atomic weight) were a periodic function of the atomic weight. He has given a more complete diagram of this in his paper to the Congrès de Physique, Paris, 1900.

Rydberg's first observation is very clearly shown on my diagrams. We learn further from them that in special families the value of v is largely dependent upon the atomic mass, and that the function is closely related to the square of the atomic mass. Plate 2, drawn from these squares, shows that the lines which connect corresponding members of homologous doublets and triplets approach one another as the atomic mass decreases and *intersect on the line of zero atomic mass.* This is true in every case studied, both in the subordinate series, with constant differences of oscillation frequency, and in the principal series of potassium, rubidium, and cæsium, and in isolated doublets.

It is evident that the points of intersection of these connecting lines cannot be absolutely determined, and a large personal element may be introduced in some cases. This personal element cannot, however, be large in the potassium and aluminium groups, where the

* 'Kongl. Svenska Vetensk. Akad. Handl.,' 1890, vol. 23, No. 11.

ordinates representing the elements with the lowest atomic masses are very close to the ordinate representing zero atomic mass.

It may be remarked that, even if it could be shown positively that the lines do not intersect on the line of zero atomic mass, they must intersect in points very near to it. Any disturbing influence must be so small that the statement may be accepted as a broad fundamental fact, as far as the elements under consideration are concerned.

(5.) The form of some of the connecting lines indicates that no element will be found of greater atomic mass than the greatest represented. This is seen most strikingly in the connecting lines of copper, silver, and gold.

The Homologous Series of Spectral Lines.

Rydberg has classed together as "Lines of Type I," the series found in the spectra of hydrogen, helium, lithium, oxygen, sodium, magnesium, aluminium, sulphur, potassium, calcium, copper, zinc, selenium, rubidium, strontium, silver, cadmium, indium, cæsium, mercury, and thallium. To these may be added gallium.

The series have been further divided into three kinds: the principal series and two subordinate series.

Only the metals of the alkalies yield principal series, and this part of the paper will deal almost wholly with these principal series. It is probable that the strongest lines of the calcium group, and the very strong doublets in the spectra of the copper group, belong to the principal series.

The Formulæ which have been Applied to Harmonic Series.

Balmer was the first to give a formula for the harmonic series of lines in the spectrum of hydrogen.* His formula is :—

$$\lambda = h \frac{m^2}{m^2 - 4}.$$

Rydberg gave† a general formula, applicable to all these series: $n = n_\infty - \frac{N_0}{(m+\mu)^2}$, where $n = 10^8 \lambda^{-1}$; n_∞ and μ are constants for each series. N_0 is a constant common to all series, and $m = 1, 2, 3\ldots$

Kayser and Runge have given the formula—

$$10^8 \lambda^{-1} = A - Bn^{-2} - Cn^{-4},$$

where $n = 3, 4, 5\ldots$ and A, B, C are constants in each series. The constants B and C apply generally to the subordinate series of monad

* 'Wied. Ann.,' vol. 25, pp. 80–87, 1885.

† *Loc. cit.*

and triad metals. The constant B is approximately equal to Rydberg's constant N_0, but a different value is used in different series.

Modifications of these formulæ have been proposed by Ames,* by Balmer,† and by Thiele.‡

The formula of Rydberg is of wide application, its form is simple, and the values of the constants are easily calculated. It has every appearance of being a more natural formula than that of Kayser and Runge. This is an important consideration, for there is doubtless some common cause acting to produce series of this type. The view has been expressed by Rydberg that: "The equation of Kayser and Runge is a formula of interpolation, which has no other object than to represent with the greatest accuracy possible the given observations." For these reasons we shall proceed to discuss it in detail, with special regard to its application to the principal series of lines.

The Constant N_0.—This constant was calculated from the lines in the hydrogen series—$N_0 = 4.10^8/h$. Rydberg has given two values: (1) In 1890, the value 109721·6; and (2) in 1900, 109675·00.

Using the figures given by Ames for the hydrogen lines,§ we have obtained the number 109706. Dyson‖ used the number 109705, and we have calculated the same number from the strongest hydrogen lines in Rowland's 'Solar Spectrum Wave-lengths.' By reducing the wave-lengths of Rowland to their corresponding values in a vacuum, we calculated the value of N_0 as 109674. It would appear that Rydberg's later value must have been obtained in this manner.

In my earlier work, the value of N_0 was taken as 109706, and in the later as 109675. The oscillation frequencies were in like manner reduced to their value in a vacuum.

The lines in the plates which accompany this paper are those given by the lower values of m in Rydberg's formula, and it is in these, especially in the strongest lines, where $m = 1$, that the formula, as used by its author, gives the greatest differences from the observed numbers. Rydberg only claims that his formula is approximately true, but the fact that it is of such general application is strong evidence for thinking it may be made more accurate. Certain facts revealed by a study of the diagrams, referred to above, and of the calculations made by Rydberg's method, indicate a modification of the formula, which will give much greater accuracy.

The Principal Series of the Metals of the Alkalies.—Each metal, except lithium, gives two principal series, the corresponding members of

* 'Wied. Ann.,' vol. 25, pp. 80–87, 1885.

† 'Verhandl. d. Naturf. Ges. in Basel,' vol. 11, pp. 448–462.

‡ 'Astrophys. Jour.,' vol. 6, p. 65.

§ 'Phil. Mag.,' vol. 30, 1890, p. 33.

‖ 'Roy. Soc. Proc.,' vol. 68, p. 33, 1901.

which form doublets. The differences in oscillation frequencies between these doublets are not constant, as in the doublets of the subordinate series; they diminish as the refrangibility increases.

The lines corresponding to the following values of m were known when this part of the work was done:

Lithium 1—9, sodium, 1—7; and potassium 1—9, rubidium 1—4, cæsium 2—7.

The differences between the observed and calculated numbers, when Rydberg's method is used, are given in the following series of potassium. The lines $m = 2—7$ were used to calculate the constants; the line $m = 1$ was omitted, so that the constants might be compared with the corresponding constants of rubidium and cæsium.

m.	Calculated number. $10^8 \lambda^{-1}$.	Observed number. $10^8 \lambda^{-1}$.	Differences.
1	13584	13042	+ 542
2	24719	24719	0
3	28991	29006	− 15
4	31069	31073	− 4
5	32233	32226	+ 7
6	32950	32940	+ 10
7	33424	33409	+ 15

Similar results were obtained with all the principal series: the differences increased slightly with the atomic mass.

Several lines have been measured in the oxyhydrogen flame spectra of the elements—potassium, rubidium, and cæsium. The results are, in some cases at least, more accurate than those of Kayser and Runge: the differences between the measurements of the same lines on different plates are smaller than the limits of error given by them. Some of the lines have not been measured before.

The oscillation frequencies, reduced to their values in a vacuum, were used in the following calculations, and the value of N_0 (109675) corresponding to this reduction was also used.

The chief points to be noted in the calculations are as follows:—

The value of the limit of the series diminishes as the refrangibility of the line, from which its position is calculated, increases, until an almost constant value is reached in the lines corresponding to values of m greater than 4. These lowest and almost constant values are, therefore, nearest to the true limits of the series.

The spectra indicate that the value of n_∞ must be the same for both series:—

m.	Differences in Oscillation Frequencies, $P_1 - P_2$.			
	Sodium.	Potassium.	Rubidium.	Cæsium.
1	17	57	225	564
2	5	19	77	181
3	—	8	35	80
4	—	5	20	40
5	—	2		
6				
7				

These figures show that the two series merge into one in both sodium and potassium, and that in rubidium and cæsium they are rapidly approaching each other. When $m = \infty$, therefore, they must have the same value, and n_∞, corresponding to this, must be common to the two series.

When we take this view it is pretty evident that the value of μ must vary throughout the series. It diminishes as the refrangibility of the line decreases, and, at the same time, the intensity of the line increases.

The differences between the values of μ for the corresponding lines of the two series of each element are very nearly constant. The figures are as follows :—

m.	Differences between values of μ . Series P_1 – Series P_2.		
	Potassium.	Rubidium.	Cæsium.
1	0·0029	0·0122 ?	0·0331 ?
2	0·0030	0·0129	0·0321
3	0·0029	0·0130	0·0320
4	0·0033	0·0132	0·0317
5	0·0022		

The mean values of these are proportional to the squares of the atomic masses, and are given by the term $18W^2 \times 10^{-7}$.

The diagrams prove that there is a very close relation between the spectra of the three elements under consideration and the atomic masses. Rydberg's equation has, therefore, been modified in accordance with the above work, and an empirical formula has been obtained, which contains only one variable, W the atomic mass. One equation, the following, gives the second principal series of all three metals with considerable accuracy :—

$$n = 35349 - 0{\cdot}2233\,W^2 - \frac{109675}{\{m + 1 + (2 - 3^{-m})\,(0{\cdot}1143 + 0{\cdot}000625\,W)\}^2}$$

The factor $(2—3^{-m})$ corrects for the variations in the fractional parts of μ, referred to above.

The first principal series are obtained by adding to $m + 1$ in the denominator the term $18\,W^2 \times 10^{-7}$.

[A closer approximation is given when, in Rydberg's general formula,

$$n = n_\infty - \frac{N}{(m+\mu)^2},$$

we substitute

$$n_\infty = 35349 - 0{\cdot}2233\,W^2; \quad N_0 = 109675,$$

and

$$\mu = \{1{\cdot}19126 + 0{\cdot}00103\,W + (0{\cdot}04377 + 13\,W^2 \times 10^{-7})\,(1 - 3^{1-m})\}.$$

These values give the second principal series of potassium, rubidium, and cæsium. To obtain the first principal series, increase the value of μ by $182\ W^2 \times 10^{-8}$. The value of n_∞, which the above gives for rubidium, is 27·5 units too great.

The wave-numbers $(10^8\lambda^{-1})$, calculated from this amended formula, are compared with the observed values in the following tables:—

Potassium.

m.	Second Series.			First Series.		
	Observed.	Calculated.	Differences.	Observed.	Calculated.	Differences.
1	*12984·5 †12980·0	12983·1	−1·4 +3·1	*13041·5 †13036·6	13038·0	−3·5 +1·4
2	24700·3	24700·5	+0·2	24719·0	24718·1	−0·9
3	28997·8	27998·4	+0·6	29006·3	29006·2	−0·1
4	31068·0	31066·8	−1·2	31072·7	31070·9	−1·8
5	*32224·0	32223·6	−0·4	*32225·8	32226·0	+0·2
6				*32939·5	32937·9	−1·6
7				*33408·5	33407·1	−1·4

* Kayser and Runge.

† Lehmann, 'Ann. d. Physik,' vol. 5, p. 633, 1901. Unmarked lines observed by the Author (Flame Spectra).

Rubidium.

m.	Second Series.			First Series.		
	Observed.	Calculated.	Differences −27·5.	Observed.	Calculated.	Differences −27·5.
1	*12575·0 †12574·3	12608·2	+5·7 +6·4	*12799·7 †12806·9	12851·9	+24·7 +17·5
2	23714·4	23738·6	−3·3	23791·4	23818·1	− 0·8
3	27832·8	27861·5	+1·2	27868·4	27897·3	+ 1·4
4	29833·5	29860·6	−0·4	29852·6	29879·8	− 0·3
5	30958	30984·9	−0·6	30968·4	30996·2	+ 0·3

Cæsium.

m.	Second Series.			First Series.		
	Observed.	Calculated.	Differences.	Observed.	Calculated.	Differences.
1	†11169·8	11170·9	+1·1	†11723·0	11718·4	−4·6
2	21764·8	21763·1	−1·7	21945·6	21944·3	−1·3
3	25707·9	25707·7	−0·2	25790·4	25790·3	−0·1
4	27635·7	27633·4	−2·3	27680·0	27677·9	−2·1
5	—	—	—	28750·3	28749·3	−1·0
6	—	—	—	29417·3	29416·7	−0·6

This formula, though empirical, involves only seven adjustable constants; in these tables it represents, in the two series, thirty-two lines. It thus affords striking evidence for the fundamental identity of type of the spectra of the three metals to which it applies, and indicates that their differences depend on the atomic mass alone. This evidence is further strengthened when it is remembered that, being only an approximation to an unknown formula, it will naturally come nearer it for large values of m than for small ones. Additional evidence supporting these views is given above by the observations upon the subordinate series of these three metals, in which the differences depend directly on the atomic mass, and upon the fundamental spectra of calcium, strontium, and barium, in which the differences depend on the square of the atomic mass.—*Added January* 27, 1902.]

Rydberg's formula and method give better results for the series

* Kayser and Runge.

† Lehmann, 'Ann. d. Physik,' vol. 5, p. 633, 1901. Unmarked lines observed by the Author (Flame Spectra).

of lines belonging to elements of low atomic mass, such as hydrogen, helium, and lithium, than for the others. The differences between the observed and calculated numbers increase in magnitude with the atomic mass.

The suggestion to take the highest value of μ, that derived from the more refrangible lines, as the starting point, appears to be perfectly fair. When this is done, we observe that the differences between the numbers calculated with this μ as a constant for the series and the observed numbers increase with the intensity of the lines. There is, it appears, some disturbing influence introduced along with the causes which increase the intensity of the lines. This disturbing influence retards the rate of vibration, and its effect becomes more marked as the atomic mass increases. It is now possible to determine the amount of the disturbance produced in this way, and to compare it with the disturbance produced by increases in the atomic mass.

The metals of the alkalies have closely allied chemical and physical properties; but the three metals, potassium, rubidium, and cæsium, are more closely related to one another than to lithium and sodium. The spectra of these three metals may be said to agree almost line for line, but the spectra of lithium and sodium are moved considerably towards the more refrangible end of the spectrum: the corresponding vibrations are much faster in lithium and sodium. The effect of increase of atomic mass in these groups is to diminish the oscillation frequency of the lines, and the rate of change is much greater in passing from lithium to sodium and potassium than from potassium to rubidium and cæsium.

These facts appear to indicate that there is something besides the molecule of the element involved in the production of the lines under consideration. The late Professor Rowland remarked, in an address on "The Highest Aim of the Physicist"* :—"We know of little or no etherial disturbance which can be set up by the motion of matter alone: the matter must be electrified, in order to have sufficient hold on the ether to communicate its motion to the ether." This fact suggests the view that the influence outside the molecule is the portion of the æther affected by the motion of the electrified molecule: that this portion of æther acts as a load on the molecule (or it may be that the molecule acts as a load on the æther), the effect in the end being that the spectral lines in the series we have been considering are produced by the mutual action of the matter and the æther.

This suggestion seems to offer an explanation for the retardation which increases with the intensity of the members of the series. It would appear that the intensity of the line is proportional to the amount of the æther affected, and as the load is greater so is the retardation of the vibrations greater.

* 'Amer. Jour. Sci.,' December, 1899, p. 406.

A supposition such as the above would imply that one molecule can produce only one line at a time. Lord Rayleigh* has already made a suggestion of this nature in discussing a formula deduced theoretically. He wrote:—"In this case instead of supposing that the whole series of lines correspond to various modes of one highly compound system, we attribute each line to a different system vibrating in a special mode."

The data available at present are far from complete, and some of the suppositions are open to question. For instance, the value of N_0 is adopted from the formula for hydrogen, without considering the influence of its atomic mass. Although the effect of this mass may be small, it may be sufficiently great to complicate the results calculated from it, so as to obscure a regular order in the changes produced by the increase in the masses of the elementary substances.

An article by Professor Pickering† on "The Spectrum of ζ Puppis," contains statements which confirm the views expressed above regarding the formula of Kayser and Runge. He has found that a slight modification of Balmer's formula $3636{\cdot}1\left(\frac{n^2}{n^2-16}\right)$, gave the lines of the ordinary series of hydrogen and the additional series more accurately than Kayser's formula, $\frac{1}{\lambda} = A + B\frac{1}{m^2} + C\frac{1}{m^4}$. He remarked: "On the whole, the observed values agree more nearly with the first formula than with the second. This is remarkable, if it does not represent the true law, since this formula contains no arbitrary constants."

A Comparative Study of the Densities and Melting Points of some Groups of the Elements.

The graphical method, employed in the investigation of spectra, has been extended to two other properties of the elements—density and melting point. These properties were chosen because the data concerning them were more complete than the data relating to other properties.

It is hoped that the graphical method of investigation may, as time and data permit, be extended to the other properties of the elements. We have observed that the shift of some of the lines in spectra is proportional to the atomic mass; of others, to the square of the atomic mass. The proposed extension of the work will tell us which properties of the elements follow the first order, which follow the second, and possibly of some which vary with some other power or function of the atomic mass. Such information will doubtless throw considerable

* 'Phil. Mag.,' vol. 44, p. 361, 1897.

† 'Astrophys. Jour.,' April, 1901, p. 232.

light on the fundamental causes of the properties of matter. The method will also be applicable to the study of compounds.

Three diagrams have been drawn and studied. They were drawn as follows:—

Abscissæ.	Ordinates.
(*a*) Densities.	Atomic masses.
(*b*) Melting points.	,, ,,
(*c*) Densities and melting points of the elements whose spectra have been studied by this method.	Squares of the atomic masses.

Diagrams (*a*) *and* (*b*).—The positions of similar elements on these diagrams may be connected by lines which resemble in form those which connect the spectra. In some groups the breaks in the lines are less marked, while in others they are more marked than in the spectra.

The changes in the diagram involving density are more regular than the changes in those involving melting points. In both of the first two diagrams lithium, beryllium, boron, and carbon lie on lines which are nearly straight.

The Metals of the Alkalies.—There is a decided break in the density diagram between sodium and potassium. A single smooth curve can be drawn through the five metals on the melting point diagram: its form and direction would be similar to those of the line joining the ends of the series in the diagram of spectra, Plate 1, of these elements.

Copper, Silver, and Gold.—Both the curves joining these metals are remarkable. The connection between the densities of sodium and copper is quite in agreement with those between magnesium and zinc, aluminium and gallium, &c. The angles which the successive lines make with the horizontal increase from the sodium–copper line to the silicon–germanium line.

There is thus an interesting double connection from sodium to potassium on the one hand, and to copper on the other.

Beryllium, Magnesium, Zinc, Cadmium, and Mercury.—The connections drawn between these are regular; from magnesium upwards the two curves are approximately symmetrical.

Beryllium, Magnesium, Calcium, Strontium, and Barium.—The connecting lines are only slightly curved. The melting points of strontium and barium are not known accurately: those recorded place them on a slightly curved line which passes on to beryllium.

The double connection from magnesium to calcium and to zinc is another interesting feature.

Boron, Aluminium, Gallium, Indium, and Thallium.—There is a break

in the density curve above aluminium, and the curve joining the metals above is nearly a parabola. In the melting point curve, boron, aluminium, and gallium are nearly on a straight line; the curve then takes a totally different form. This break corresponds to the break in the diffuse series of the spectra, so that the same fundamental causes appear to act in the production of these series, and in determining the densities and melting points of the elements. It will be an interesting study to trace those properties of the elements which vary regularly from aluminium to thallium in the way that the sharp series vary.

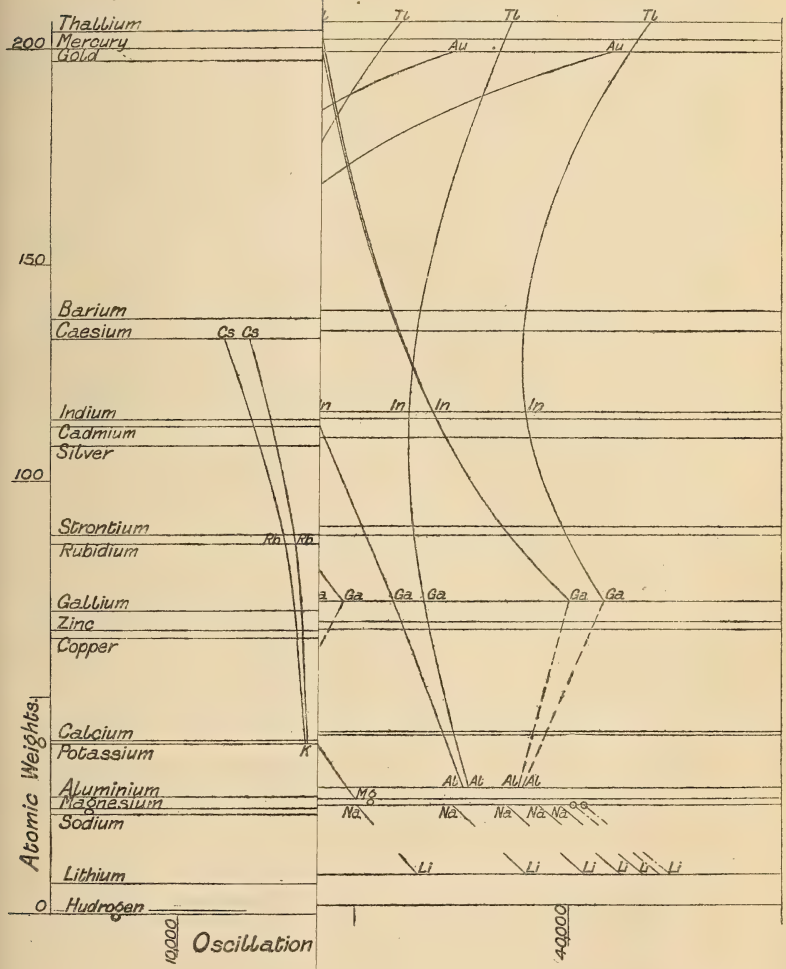
Silicon, Germanium, Tin, and Lead.—The connecting lines between these elements are peculiar. There is a suggestion that the density of germanium is too low: the diagram indicates that it should be between 5·8 and 5·9 instead of being 5·47. We observe, also, that the melting point is remarkable. If this followed the order of the other cases studied it would be below the melting point of tin. It is possible, therefore, that germanium is a peculiar element, and worthy of more complete investigation. From carbon the melting point falls with increase in the atomic weight until tin is reached; and it is only then the change of direction takes place, one stage later than in the other groups.

Diagram (*c*).—Three of the long connecting lines approach to straight lines in this diagram: those in both density and melting point diagrams for zinc, cadmium, and mercury, and that in the density diagram for gallium, indium, and thallium. The first of these three lines is almost perfectly straight, and the densities may be calculated from an equation containing the square of the atomic mass: Density $= 6{\cdot}38 + 1803\,W^2 \times 10^{-7}$. This formula gives zinc and mercury correctly, and cadmium 0·01 too high.

Conclusions.

It has been usual for investigators to rest satisfied when the properties of the elements were shown to be "a periodic function of the atomic mass." Diagrams drawn, by the method employed in this paper, of each of the properties of the elements will show in what degree the properties vary with the atomic mass, and will make it easier to establish the exact quantitative relations.

The work and results now presented indicate that the properties of the elements are fundamentally due to the structure of the atoms, as revealed by their spectra, rather than to the quantity of matter in them. It seems, for instance, inconceivable that the transition from calcium to strontium proceeded through the intermediate elements, when we consider that the strontium molecules must have a similar structure to those of calcium. This structure is so simple that the fundamental (Bunsen flame) spectrum of each of these elements consists of a single line. The anomalies, according to Mendeléeff's law, in the atomic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Mercury
Gold
200
150
Barium
Caesium
Indium
Cadmium
Silver
100
Strontium
Rubidium
Gallium
Zinc
Copper
Calcium
Potassium
Aluminium
Magnesium
Sodium
Lithium
Hydrogen
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Atomic Weights
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Oscillation
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In
Rb
Ga
K
Al
Mg
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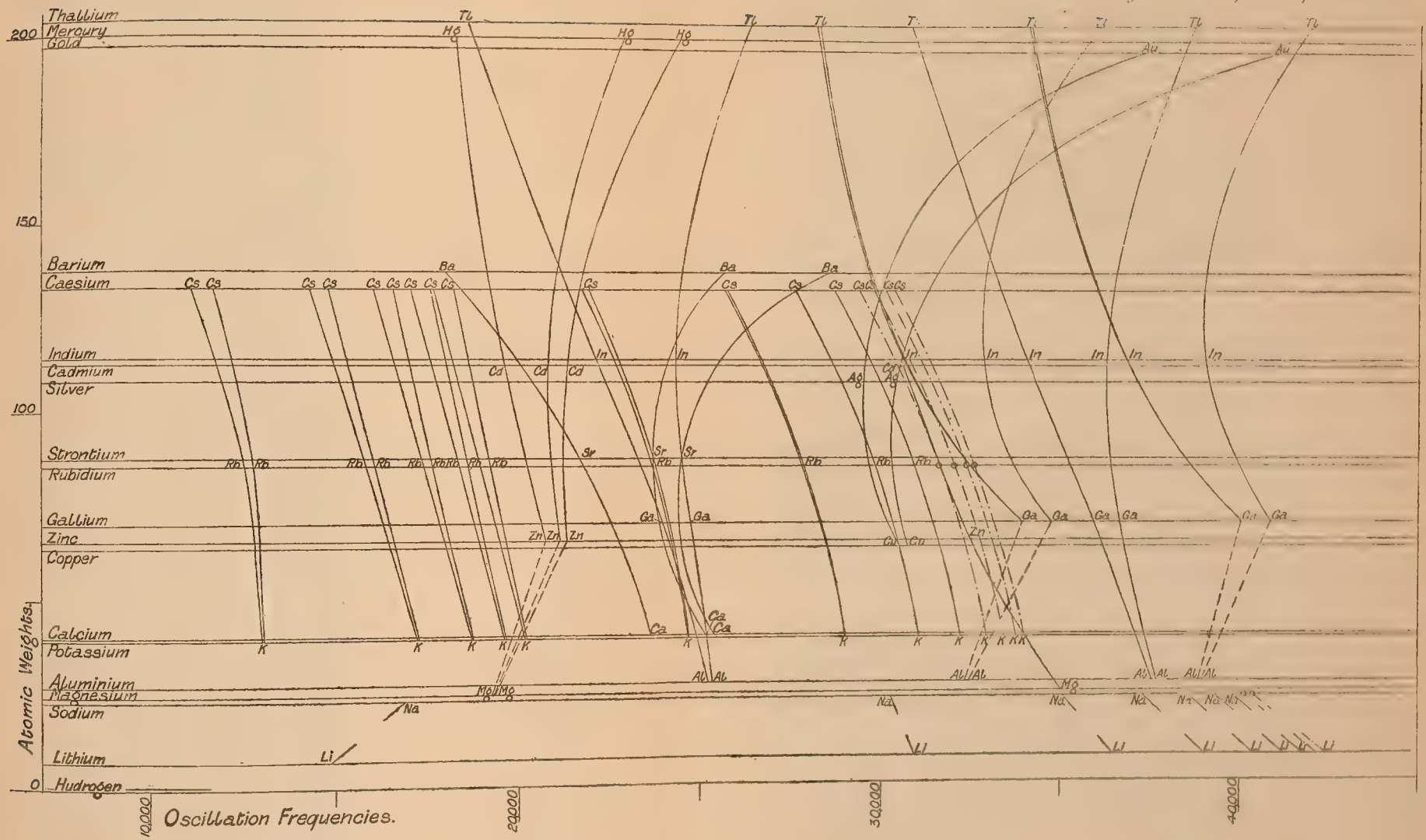
Li


Thallium
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Cadmium
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Strontium
Rubidium
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Magnesium
Sodium
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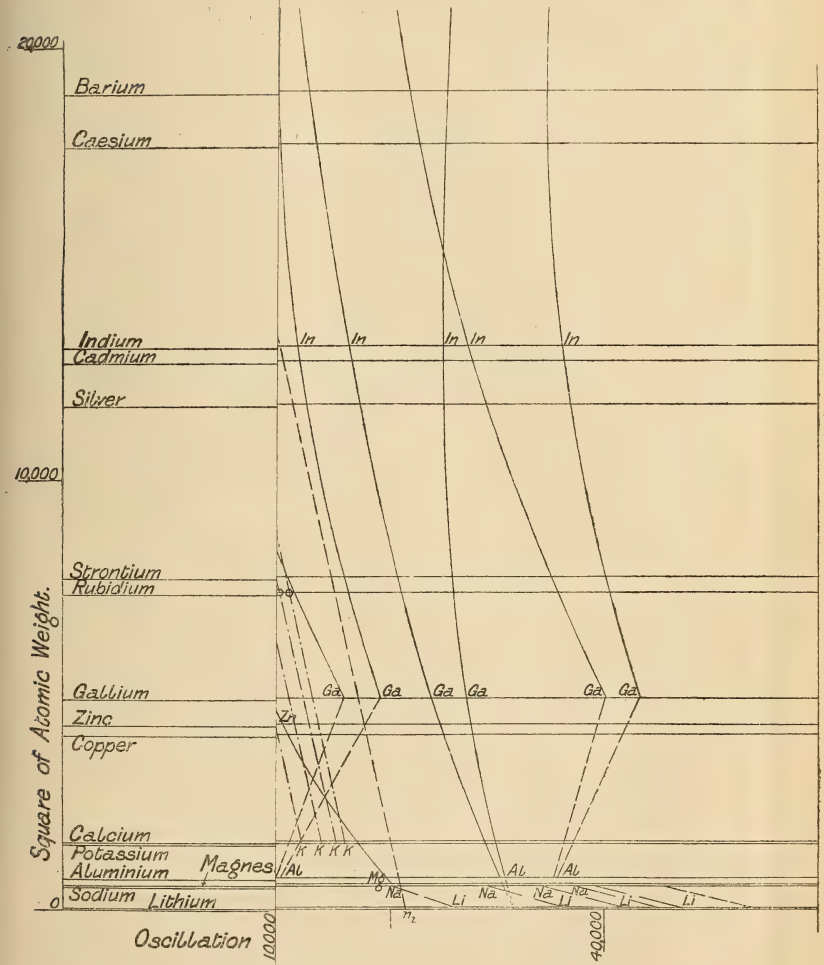
Oscillation Frequencies.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Square of Atomic Weight.
Barium
Caesium
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Cadmium
Silver
Strontium
Rubidium
Gallium
Zinc
Copper
Calcium
Potassium
Aluminium
Sodium
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Magnes
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Ga
Zn
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Al
Mg
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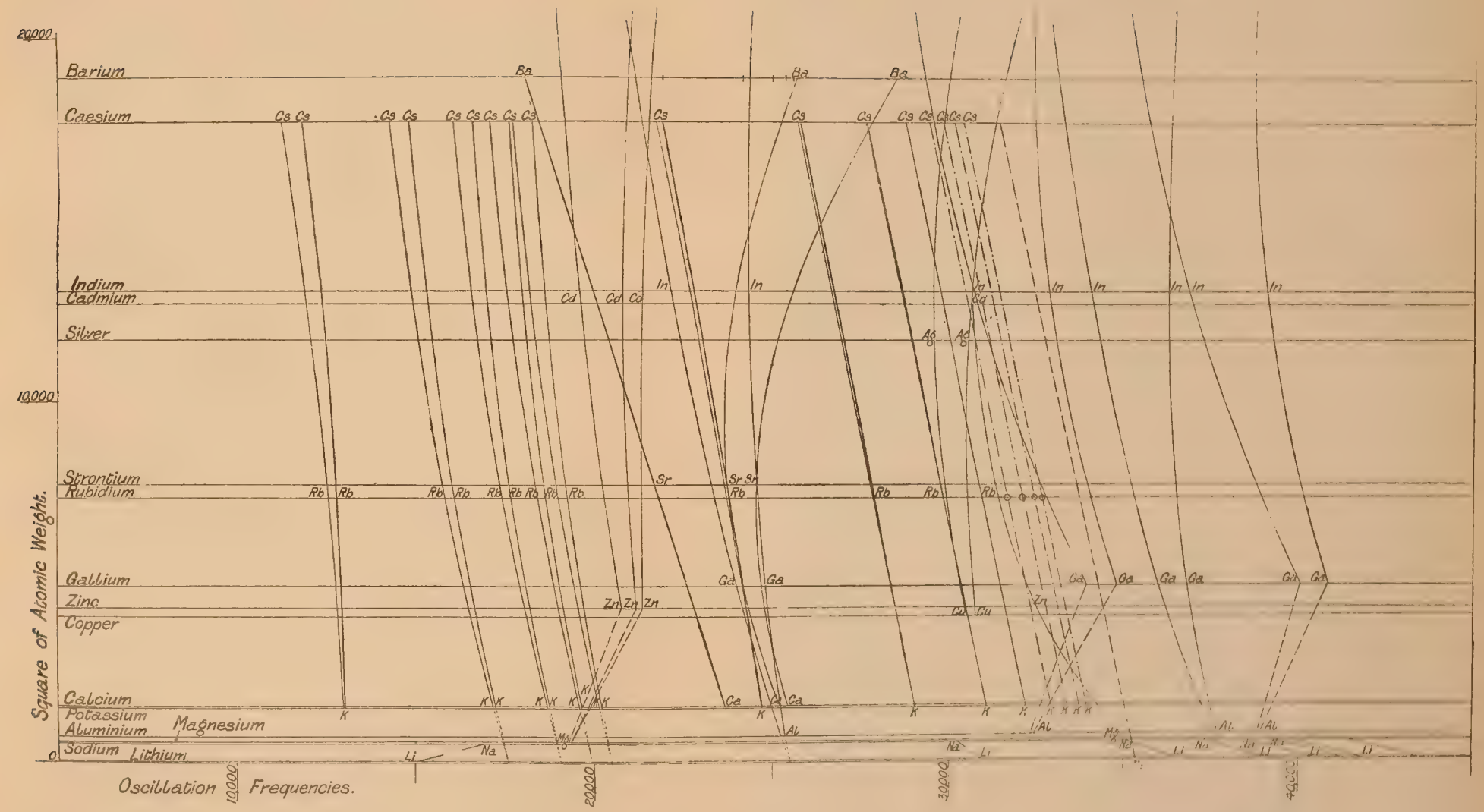
Li
10000
40,000
Oscillation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Square of Atomic Weight.
Barium
Caesium
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Cadmium
Silver
Strontium
Rubidium
Gallium
Zinc
Copper
Calcium
Potassium
Aluminium
Magnesium
Sodium
Lithium
10,000
20,000
30,000
40,000
Oscillation Frequencies.

masses of tellurium and iodine, &c., are further evidences of this. The properties of tellurium and iodine may have nothing whatever to do with each other. They are, however, closely related to, and in correct order with, those of the elements of their respective groups as given above. The genesis was not in the direction of tellurium to iodine, but from, or perhaps through, oxygen and fluorine respectively. So also with regard to the other groups.

It is more probable that in the genesis of the elements the properties of certain fundamental substances are modified by successive additions of matter to them,* or by causes of which this is, to us, the apparent result. The regularity in the changes in the properties of lithium, beryllium, boron, and carbon, as seen in the diagrams, is very remarkable. It is, furthermore, very suggestive, for the changes in properties are approximately proportional to the quantity of matter in the atom in excess of a constant (which is about 6), as if it were the same matter that is added in each case.

I must express my best thanks to Dr. J. H. Vincent, to Professor Liveing, Dr. Larmor, and Professor J. J. Thomson, for the interest they have taken in this work, and for the kindly encouragement they gave me, especially in the earlier stages of it. It was while working with Professor Hartley that I acquired the knowledge of spectra which led me to begin the investigation, and I am deeply grateful to him for the means of acquiring that knowledge.

"Experimental Researches on Drawn Steel.—Part I. Magnetism and its Changes with Temperature.—Part II. Resistivity, Elasticity and Density, and the Temperature Coefficients of Resistivity and Elasticity." By J. Reginald Ashworth. Communicated by Professor Schuster, F.R.S. Received January 30,—Read March 6, 1902.

(Abstract.)

Part I.

In a former paper it was shown that the variation of the intensity of magnetisation of a magnet under fluctuations of temperature is controlled to a large extent by the self-demagnetising factor of the

* *January* 27, 1902.—Professor Hartley has recently called my attention to a letter "On the Inadequacy of Aids and Facilities for Scientific Research," which he wrote to the 'Chemical News' on November 9, 1895. The following statement is quoted from that letter:—"One element in a group differs in its properties from another, not because it consists of another kind of matter, but because the quantity of matter in an atom of it is different."

magnet, the self-demagnetising factor being governed by the dimension ratio. In general, an increment of temperature in the cyclic state reduces and a fall of temperature augments the magnetic intensity, so that α in the equation

$$I_{t'} = I_t(1 + \alpha \overline{t' - t})$$

is negative, t' being a greater temperature than t and $I_{t'}$ and I_t the corresponding magnetic intensities. But it was discovered that magnets made of pianoforte wire in the commercial state, more than 50 diameters long, exhibited a positive or incremental coefficient. If the magnet were much shorter the coefficient was decremental, and, for an intermediate dimension ratio, zero.

In the present paper a complete experimental investigation of the temperature coefficient of a magnet is undertaken. Attention is at first confined to pianoforte steel wire, and tables and curves are given of the change of α, and the change also of I, as the dimension ratio is increased from about 16 to 100 for the wire in three conditions, namely, annealed, glass hard, and commercial drawn state.

In the first two the coefficient is decremental and, in magnitude, large for annealed and very small for glass hard; but in the drawn state α changes from decremental to incremental as the dimension ratio advances. It thus appears that the drawing is responsible for the peculiar behaviour of α.

To confirm this, experiments were made on twelve samples of pianoforte steel, representing every stage in the process of drawing from the rolled rod through annealing and tempering to the fine-drawn wire, the drawing being carried one or two steps farther than is usual in commercial practice. The experiments were made on lengths of 50 and 100 diameters. The curves which are traced show very clearly that α changes in sign at a not very advanced stage in the drawing, grows more positive, reaches a maximum, and then droops towards zero again at extreme drawing. The magnetic intensity rises steadily with drawing, and shows no tendency to diminish even at the final stage of drawing; it is then about 200 per cent. greater than at the first drawing.

An examination of a piece of drawn steel wire about 400 diameters long then follows, in which susceptibility and intensity are determined when the wire was at air temperature and when it was at 100° C.

The effect of heating is very pronounced and, unlike the general behaviour of iron and steel, the hot curve of magnetisation is always above the cold curve, as the steel is carried to its greatest intensity and brought to its residual condition by the withdrawal of all force. Thus it retains more magnetism hot than cold. But on demagnetising, the hot curve droops faster than the cold curve, and the curves intersect when the reversed force is about 4 C.G.S. units, and at this

point the magnetic intensity is the same hot or cold. The explanation of the fact that a self-demagnetising force of given amount produces a zero temperature coefficient in drawn steel now becomes clear.

The influence of magnetic intensity on the magnitude of α is next investigated in a long series of observations. The wire was magnetised step by step, and at each step the temperature coefficient and the percentage permanent change of intensity were determined, both for induced and residual intensity. The temperature coefficient in general follows the susceptibility and becomes least at the highest intensity but always incremental; on demagnetising it increases slightly and then falls towards zero, which occurs at some very low residual intensity.

The behaviour of the residual magnetism as demagnetisation proceeds is very interesting; when a small part of the magnetism is removed, the effect of alternate heatings and coolings is to leave the intensity higher than before the application of heat and cold, and the more the magnetism is removed the greater the recovery by heating and cooling; and, at last, on applying so large a demagnetising force as to remove all the magnetism and to leave a small residual intensity inverse to the direction of the original intensity, then, heatings and coolings clear this out and restore some of the original magnetisation. The behaviour of the coefficient during these changes is also discussed.

For comparison, the same series of experiments was performed upon an annealed iron wire about 400 diameters long. The hot curve of magnetisation crosses the cold curve a little beyond maximum susceptibility and the maximum residual intensity is less hot than cold; it would therefore appear that at some small initial intensity the coefficient should be positive, at a later stage zero, and finally negative. The zero coefficient was not obtained in these experiments, but the subsequent negative coefficient was traced, and it was found that when the susceptibility is large the negative coefficient is a minimum; the coefficient is throughout negative during demagnetisation.

The intensity under heating and cooling is similar in behaviour to the intensity of drawn steel; on magnetising, heatings and coolings always cause a loss of residual magnetism, but on demagnetising heatings and coolings restore some of the magnetism removed. It would no doubt be advantageous for the sake of producing a magnet of constant intensity to apply a small reversed force after magnetising to saturation, any fluctuations of temperature afterwards would then tend to increase rather than diminish the intensity.

It is pointed out that the gradual decay of magnetism in observatory magnets will generally be accompanied by some change in the coefficient, and that remagnetisation of magnets will likewise alter the magnitude of the coefficient; the general effect of this change will be

determined by the position of the point of intersection of the hot and cold curves of magnetisation.

Observations, extending over several years, then follow on four magnets made of drawn steel and treated in two different ways so as to have nearly zero coefficients, and the effects of time and the effects of remagnetisation on the temperature coefficient and on the intensity are studied.

Part II.

The series of wires representing the twelve stages in the manufacture of drawn wire were subjected to examination for resistivity and its temperature coefficient, for Young's modulus and its temperature coefficient, and for density.

Resistivity is augmented by tempering but diminished by drawing up to a certain stage; extreme traction, however, causes a rapid increase again of resistivity.

The temperature coefficient is related to the resistivity, so that, in general, high resistivity and low coefficient are associated. Drawing, however, does not produce a very large change in the temperature coefficient of resistivity.

Young's modulus increases markedly with tempering and to a less extent with drawing up to the point where the resistivity is a minimum; after that, extreme drawing produces a sharp diminution of the modulus.

The temperature coefficient of Young's modulus presents an interesting relation to the modulus and like the resistivity coefficient follows in an inverse sense the modulus curve, so that when the modulus is large the coefficient is small.

It was observed that after heating and cooling the wire, the value of Young's modulus did not return to its initial value but was always a little greater; perhaps this was not a permanent effect, although it persisted for some time.

A careful series of determinations on the density of the wires showed that, with some initial irregularities, the density is very decidedly increased by drawing to the very last stage, when it becomes 8 grammes per c.cm.

There is a very close correspondence between density and magnetic intensity, the two being nearly proportional to one another over a considerable range; there is also some evidence of agreement between a smoothed curve of Young's modulus and the magnetic intensity; both appear to be functions of the density.

"On the Spark Discharge from Metallic Poles in Water." By Sir NORMAN LOCKYER, K.C.B., F.R.S. Received January 31, —Read March 6, 1902.

[PLATE 3.]

During the appearance of the new star in the constellation Auriga, which was discovered in January, 1892, the Kensington photographs were the first* to show that several of the brighter lines were accompanied by absorption lines on their more refrangible sides.

This appearance I explained on the hypothesis that we were dealing with at least two bodies, one giving a radiation, and the other an absorption spectrum, the differential movements of which could be determined by the changes of wave-lengths observed.

In a paper† published in the year 1899, Dr. J. Wilsing made the suggestion that, in view of the great velocities shown by the large displacement of the lines in the spectra of new stars, and the occurrence of these displacements in the same direction, some other cause of them was probably at work, and he suggested that the cause might be high pressure, which drives the line towards the red.

The First Observations of Non-symmetrical Emission.

The non-symmetrical development of emission lines is of frequent occurrence in ordinary arc spectra. Typical photographs of such phenomena were referred to by me in illustration of papers communicated to the Royal Society more than a quarter of a century ago on peculiarities of emission and absorption spectra.‡

The following extracts from parts of these communications will serve to indicate the facts observed at that time :—

"*Photographs showing Non-symmetrical Lines.*

"I. Spectrum showing two Ag lines at about wave-lengths 4054·3 and 4210·0. Both lines are fluffy and reversed; the less refrangible line is much more strongly expanded on its more refrangible side, and is carried up to a much greater height as a radiation line than its other side. The more refrangible line is more symmetrical, but presents the same phenomenon to some extent, only in the opposite direction, its less refrangible side being the most developed.

"II. Spectrum of Rb, showing line at wave-length 4202. Here the two ends of the line are produced by radiation alone, the central

* 'Roy. Soc. Proc.,' vol. 50, p. 434.

† 'Astrophys. Journ.,' vol. 10, p. 113, 1899.

‡ 'Phil. Trans.,' vol. 164, Part II, pp. 805–813, 1874; 'Roy. Soc. Proc.,' vol. 28, pp. 428–432, 1879.

portion showing absorption on its more refrangible side, with fluffy shading on its less refrangible side."

Afterwards, when higher dispersions became available, the investigations of Messrs. Humphreys and Mohler on the effect of pressure on spectrum lines* showed that the actual wave-length of a line was increased by pressure; thus Humphreys† states "the wave-lengths of all fine and sharp lines, *and also of the reversals of heavy ones*, increase with increase of pressure around the arc, no matter how the lines may spread out, symmetrically or chiefly towards either side."

In the case of pressures of twelve atmospheres, a shift of scarcely 0·05 tenth metre was observed by Messrs. Humphreys and Mohler. Eder and Valenta‡ in their work on the spark spectra of argon and sulphur under pressure obtained a displacement amounting to as much as one tenth-metre. With flame spectra of the easily volatile metallic salts, small displacements, averaging 0·4 tenth metre, were observed by Ebert,§ and were explained by him as being due to an unsymmetrical broadening of the lines towards the red.

Dr. Wilsing thought that such investigations suggested|| "the direction which must be taken in the experiments for producing shifts of lines without motion in the sight line, and ultimately for producing double spectra."

Wishing to avoid the experimental difficulties necessarily connected with the employment of high pressures, he made use of the fact that very high tensions are produced when electric *sparks* are discharged in liquids.

He employed a large induction coil, with a spark gap inserted in the secondary circuit, in connection with a battery. With the passage of each spark "a blinding discharge took place between the electrodes in the water, giving a very intense continuous spectrum crossed by faint lines." The discharge spectra in water and air were photographed on the same plate with a spectrograph, the scale of the spectrum being about 50 mm. between λ 4800 and λ 4600, and the accuracy of the determination of the wave-lengths of the sharp lines could be obtained within a few hundredths of a tenth-metre. Further, several plates were employed which were secured with a grating spectrograph of high dispersion, and with a large prism spectrograph.

Dr. Wilsing investigated in this way the spectra of the metals iron, nickel, platinum, copper, tin, zinc, cadmium, lead, and silver, and arrived at the conclusion that "there now occur displacements of lines

* 'Astrophys. Journ.,' vol. 3, pp. 114–135, 1896; vol. 4, pp. 175–181, 249–262 (1896); vol. 6, pp. 169–232, 1897.

† 'Astrophys. Journ.,' vol. 6, p. 183.

‡ 'Denkschriften der K. akad. der Wiss. zu Wien.,' vol. 64, pp. 1–39, vol. 67, pp. 97–151.

§ 'Wied. Ann.,' vol. 34, pp. 34–90, 1888.

|| 'Astrophys. Journ.,' vol. 10, p. 115.

and double lines which are in every respect similar to those in the spectra of Nova Aurigæ." Pressure, then, according to Dr. Wilsing, is the cause of the duplication and broadening of the lines in the spectra of new stars.

The great importance of this result for stellar spectroscopy rendered it imperative to repeat the experiments, and I at once commenced them, using the large Spottiswoode coil, capable of giving a 42-inch spark in air, controlled by placing a large glass plate-condenser in the secondary circuit, so that a spark of length 3 mm. was obtained in air, and about 0·5 mm. in water. The photographs of the more intense lines in the water-spark spectrum showed very distinct reversals.

The work was postponed a little later owing to this coil being no longer available, but it was again resumed with a smaller (10-inch) coil while waiting for a new large one which is under construction.

With this coil the investigation has been extended by photographing the spark spectra of several other metals in water, and these have furnished material for a more general classification of the attendant phenomena.

The coil used for producing the discharge being capable of giving only a 10-inch spark, had a 1-gallon Leyden jar placed in parallel with the secondary circuit. The spectrograph employed was a large concave Rowland grating of 6 inches diameter, ruled with 14,438 lines to the inch, and having a radius of curvature of 21 feet 6 inches. The first-order spectrum was employed, arranged to photograph the region of the spectrum from λ 3800 to λ 4800, occupying a length of 18 inches on the plate. Distilled water was used in all cases.

Of the metals so far examined (iron, silver, lead, copper, zinc, and magnesium), only three—iron, zinc, and magnesium—show reversals of the principal lines, and those of zinc are very weak.

In all cases the lines of the spectrum of the spark in water are much broader than the corresponding lines in the spectrum of the air-spark. From an examination of the different photographs, however, showing many lines of varying degrees of intensity, it appears that the broadening is, for the most part, of a similar nature to that observed in the arc spectrum in air when an excess of material is introduced between the poles.

The Phenomena presented by the Spark in Water.

(*a.*) *General.*

In the cases of iron and magnesium, many lines undergo complete reversal, for example, the following :—

Iron.		Magnesium.
4045·98	4325·94	3829·50
4063·76	4383·72	3832·45
4071·91	4404·93	3838·44
4271·93	4415·29	
4308·06		

As shown by the enlargements, this reversal is not always symmetrical with the original bright line, and the part of the emission line on the red side of the reversal is the brighter. It will be evident that in such cases if the exposure is insufficient for the less intense component to be photographed, the appearance of a bright line in a position greatly displaced towards the red will be presented, as is shown in the line of iron at λ 4260·64.

In the case of copper, we have stopped apparently at such an intermediate stage, and the phenomena observed thus appear to agree more closely with those described by Dr. Wilsing. In this case no reversals have actually taken place, and the only lines seen in the water-spark spectrum present the appearance of broad bands, considerably displaced towards the red, and having their more refrangible edges rather sharply defined by absorption, which is not otherwise manifested, while the less refrangible edges are very diffuse.

With zinc two of the lines in the strong group of three in the blue-green region show reversal, the absorption line being nearly normal, separating parts of the emission line of very different intensities. These lines, λ 4722·34 and λ 4810·72, are much more intense on the red side of the central absorption line. In the remaining line of the triplet at λ 4680·32 there is no reversal, but the maximum of intensity of the emission line is also shifted towards the red.

(*b.*) *Classification of the Different Phenomena presented.*

Considering the photographs obtained with various exposures and conditions, the phenomena observed may be grouped as follows :—

(1.) Broadened bright line.
(2.) Broadened bright line with central absorption line.
(3.) Broadened bright line with non-symmetrical absorption (maximum of emission towards red).

(1.) *Broadened Bright Line.*—This appearance is well shown in the spectrum of copper and the under-exposed spectrum of iron.

The broadened line is not of uniform intensity throughout its breadth, being stronger on the blue side, which is terminated almost abruptly, while the broader towards the red is more diffuse.

(2.) *Broadened Bright Lines with Central Absorption.*—This is well shown in the central line of the violet triplet of iron at λ 4063·76.

(3.) *Broadened Bright Line with Non-symmetrical Absorption (Maximum of Emission towards Red).*—The best examples obtained of this type of reversal are in the spectra of iron. The strong line at λ 4260·64 in the water-spark shows the most decided asymmetry, the less refrangible component of the underlying bright line being 7 or 8 times stronger than the part on the violet side of the absorption line. There appears to be no suggestion, either, of the line being duplex, so that the asymmetery cannot be explained as due to the interaction of two neighbouring reversals of varying intensities.

In the case of the absence of any line at 4481·30 in the spark in water, it may not be owing to the balance of absorption and radiation, but to a special peculiarity of this line. From many considerations, I regard 4481·30 as a high temperature line only, and therefore it may be that the cooling action of the water envelope surrounding the water-spark entirely prevents the production of this radiation.

From these considerations it appears evident that, if proper exposures be given, lines may be photographed in the spectrum of iron, say, which show all the phenomena described by Dr. Wilsing, but so related to each other and the complete stage—that of reversal, symmetrical or unsymmetrical—that it is impossible to regard them as anything abnormal. A typical set of lines illustrating these points, beginning with complete reversal with maximum of emission towards red, is as follows :—

Type.	Example.	Remarks.
1. Complete reversal (strong)..	4415·29 (Fe)	Both components of bright line shown strongly, red side most prominent.
2. Complete reversal (weak) ..	4415·29 (Fe) (another photograph)	Both components of bright line shown, red side much the stronger.
3. Partial reversal (weak) ..	4282·57 (Fe)	Appearance of a bright line with a dark border on more refrangible side.
4. Partial reversal (weaker) ..	4282·57 (Fe) (another photograph)	Bright component predominant, dark line only just visible.
5. No reversal	4315·26 (Fe) and many other weak lines.	—

(c.) *Variation of Intensities.*

The most prominent lines in the water-spark are not always the chief lines of the air-spark. This is well shown in the spectra of iron and copper.

Many of the lines in the spark of iron, if their intensities are compared under the two conditions of sparking, show distinct inversions. A typical instance of this occurs with the lines at λλ 4422·74 and 4427·48. With the spark in air λ 4427·48 is quite twice as strong as λ 4422·74, whereas in the water-spark there is scarcely any trace of a line at λ 4427·48, the 4422·74 line being, however, easily seen.

Another example, slightly less prominent, is found in the lines at λλ 4315·26 and 4337·22. With the spark in air these lines are almost equal in intensity, but in the water-spark λ 4315·26 has about three times the intensity of λ 4337·22.

In the case of copper, in the ordinary spark the most prominent lines are those at λ 4275·32 and λ 4651·31. In the water-spark spectrum the line at λ 4587·19 is almost as strong as either of the lines just mentioned, although in the ordinary spark it is much weaker.

Application to Stellar Spectra.

I will next consider the bearing of these results on the explanation of certain features of the spectrum which is characteristic of new stars. It has been seen that in the water-spark the position of the absorption undergoes little if any change of position, while in the case of non-symmetrical reversals, a bright line may be observed greatly displaced towards the red. In the new stars, on the other hand, the absorption lines are greatly displaced, the accompanying bright lines occupying in comparison normal positions. The facts are as follows:—

In the case of Nova Aurigæ the emission lines had practically normal wave-lengths, but the displacements of the dark lines at H_ϵ was about 10·7 tenth-metres towards the violet, indicating a velocity of approach of about 500 miles per second.

The recent new star in Perseus exhibited the same normal positions of the bright lines, and indications of even greater displacements of the dark lines, at one time amounting to 15 tenth-metres at H_ϵ, representing a velocity of approach of the body producing the dark-line spectrum of over 700 miles per second.

These values differ enormously from those produced by pressure. The amount of shift produced by subjecting the light source to pressure is given by Humphreys and Mohler, in the paper above referred to, as follows:—

Fig. 1.

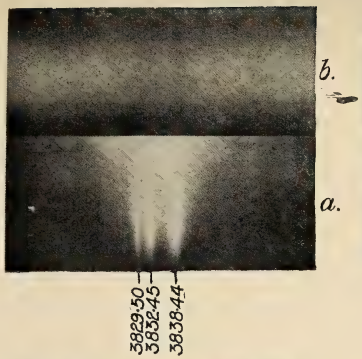
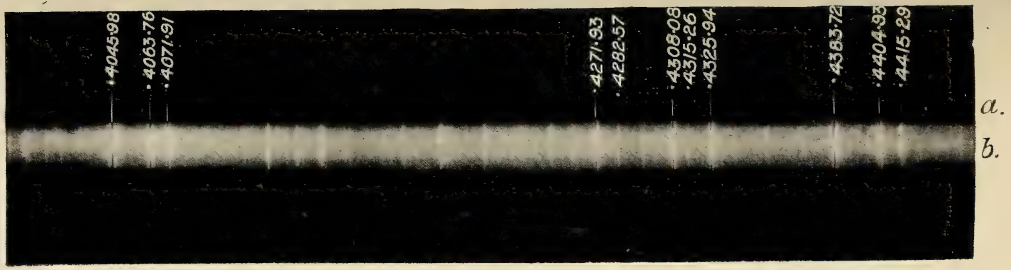


Fig. 2.



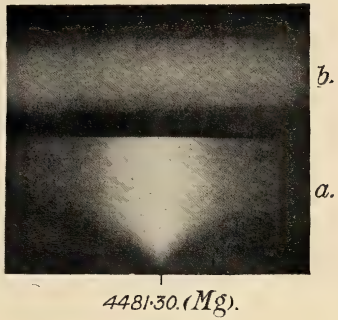


Fig. 3.

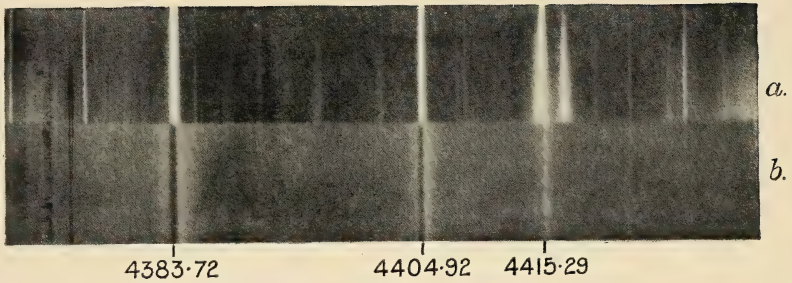


Fig. 4.

Fig. 1. Ultra-violet triplet of magnesium.
Fig. 3. Blue line of magnesium.
Fig. 2. Blue and violet lines of iron.
Fig. 4. Blue triplet of iron.

In all cases a = air spark, b = water spark.

λ	Shift in tenth-metres.	Atmospheres.
4045·98	0·009	6
4045·98	0·020	$11\frac{1}{4}$
4383·72	0·016	$9\frac{3}{4}$
4383·72	0·026	$11\frac{1}{2}$

We find then that the known direct effect of pressure on the radiation or absorption lines is the same, in quality, in water as in air, that is, we get displacements in the *opposite* direction to that we observe the dark lines to occupy in the spectra of Novæ, and we find further that the amount of shift observed in the spectra of new stars differs not only in this respect but also in degree, thus:—

Spark in water.	New stars.
1. Absorption lines least shifted.	Absorption lines most shifted.
2. Radiation lines most shifted.	Radiation lines least shifted.
3. Absorption shift small.	Absorption shift enormous.

It would thus appear that the pairs of bright and dark lines shown in the spectra of new stars do not arise from the cause which produces the appearances presented in the spectrum of the spark in water.

My thanks are due to Mr. C. P. Butler, who obtained and discussed the photographs of the spark spectra, and who, together with Dr. Lockyer, assisted me in the preparation of the paper, and to Mr. F. E. Baxandall, who checked the wave-lengths of the lines discussed and studied the behaviour of the lines representative of the different phenomena.

"The Differential Equations of Fresnel's Polarisation-vector, with an Extension to the Case of Active Media." By James Walker, M.A. Communicated by Professor Clifton, F.R.S.

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1. In many problems of physical optics it becomes necessary to know the differential equations that the polarisation-vector of a stream of light has to satisfy, and the boundary conditions that subsist at the interface of media possessing different optical properties.

These are usually obtained by formulating some theory respecting the character of the ether in the media and the nature of the vibrations in a train of waves, but there is an obvious advantage in directly basing our investigations, if possible, on the known experimental laws of the propagation of a luminous disturbance.

This has been done by Voigt* in the case of an ordinary isotropic medium by using the principle of interference that lies at the very basis of the science of physical optics, combined with the fact that the propagational speed of light is independent of the direction of the waves; from the equations thus obtained he then forms an expression that may be regarded as representing the energy of the luminous disturbance, and generalising this he deduces by the principle of least action the equations that refer to other classes of homogeneous media.

There is, perhaps, something artifical in this extension of the expression for the energy, and it is therefore better, when this can be done, to apply to each separate case the method employed by Voigt for isotropic media.

2. This plan of procedure presents no difficulty in the case of ordinary crystalline media. According to Fresnel's laws of double refraction, the polarisation-vectors of the waves that can be propagated in any given direction, are parallel to the axes of the central section of a certain ellipsoid—the ellipsoid of polarisation—parallel to the plane of the waves, and the propagational speeds of the corresponding waves are given by the inverse of the lengths of these axes.

If then the equation of the ellipsoid be

$$a_{11}x^2 + a_{22}y^2 + a_{33}z^2 + 2a_{23}yz + 2a_{13}zx + 2a_{12}xy = 1 \quad \ldots\ldots \quad (1),$$

we obtain by the ordinary methods of determining maxima and minima

$$\left.\begin{aligned} (a_{11} - \omega^2)\,\alpha + a_{12}\beta + a_{13}\gamma &= \mathrm{F}l, \\ a_{12}\alpha + (a_{22} - \omega^2)\,\beta + a_{23}\gamma &= \mathrm{F}m, \\ a_{13}\alpha + a_{23}\beta + (a_{33} - \omega^2)\,\gamma &= \mathrm{F}n, \end{aligned}\right\} \ldots\ldots\ldots\ldots\ldots \quad (2),$$

where

$$\mathrm{F} = (a_{11}\alpha + a_{12}\beta + a_{13}\gamma)\,l + (a_{12}\alpha + a_{22}\beta + a_{23}\gamma)\,m + (a_{13}\alpha + a_{23}\beta + a_{33}\gamma)\,n \quad \ldots\ldots\ldots \quad (3),$$

(l, m, n) being the direction-cosines of the normal, ω the propagational speed of the wave, and (α, β, γ) the direction-cosines of its polarisation-vector.

Now if (u, v, w) be the components of the polarisation-vector, the principle of interference is expressed by

$$u = \Sigma\alpha\mathrm{D}, \quad v = \Sigma\beta\mathrm{D}, \quad w = \Sigma\gamma\mathrm{D}, \quad \mathrm{D} = \mathrm{A}\,.\,\mathrm{Exp.}\{\iota\kappa\,(lx + my + nz - \omega t),$$

* 'Kompendium der Theoretischen Physik,' vol. 2, part V, §§ 6, 7.

where $\kappa = 2\pi/\lambda$, λ being the wave-length, and the differential equations of the vector are obtained by eliminating the exponentials and the direction-cosines from these expressions by the aid of equations (2).

This gives at once

$$(\ddot{u}, \ddot{v}, \ddot{w}) = \nabla^2\left(\frac{\partial}{\partial u}, \frac{\partial}{\partial v}, \frac{\partial}{\partial w}\right)\Omega$$

$$-\left(\frac{\partial}{\partial x}, \frac{\partial}{\partial y}, \frac{\partial}{\partial z}\right)\left(\frac{\partial}{\partial x}\cdot\frac{\partial\Omega}{\partial u}+\frac{\partial}{\partial y}\cdot\frac{\partial\Omega}{\partial v}+\frac{\partial}{\partial z}\cdot\frac{\partial\Omega}{\partial w}\right) \quad \text{.....} \quad (4),$$

where

$$2\Omega = a_{11}u^2 + a_{22}v^2 + a_{33}w^2 + 2a_{23}vw + 2a_{13}wu + 2a_{12}uv \quad \text{.....} \quad (5).$$

If we introduce a new vector ϖ, the time-gradient of which is defined by

$$(\dot{\varpi}_1, \dot{\varpi}_2, \dot{\varpi}_3)$$

$$= \left(\frac{\partial}{\partial y}\cdot\frac{\partial\Omega}{\partial w}-\frac{\partial}{\partial z}\cdot\frac{\partial\Omega}{\partial v}, \frac{\partial}{\partial z}\cdot\frac{\partial\Omega}{\partial u}-\frac{\partial}{\partial x}\cdot\frac{\partial\Omega}{\partial w}, \frac{\partial}{\partial x}\cdot\frac{\partial\Omega}{\partial v}-\frac{\partial}{\partial y}\cdot\frac{\partial\Omega}{\partial u}\right) \quad \text{...} \quad (6),$$

equation (4) may be written

$$(\dot{u}, \dot{v}, \dot{w}) = -\left(\frac{\partial\varpi_3}{\partial y}-\frac{\partial\varpi_2}{\partial z}, \frac{\partial\varpi_1}{\partial z}-\frac{\partial\varpi_3}{\partial x}, \frac{\partial\varpi_2}{\partial x}-\frac{\partial\varpi_1}{\partial y}\right) \quad \text{.....} \quad (7).$$

Thus Fresnel's theory of double refraction leads to the consideration of three vectors—

(1.) The polarisation-vector D with components u, v, w.

(2.) A vector E with components $\partial\Omega/\partial u$, $\partial\Omega/\partial v$, $\partial\Omega/\partial w$.

(3.) A vector ϖ, such that $\dot{\varpi}$ = curl E,

and D and ϖ are connected by the relation, $\dot{\mathrm{D}} = -$ curl ϖ.

Also the vectors D and ϖ are perpendicular to one another and in the plane of the wave, and the vector E is perpendicular to the vector ϖ and in the direction of the normal to the ellipsoid of polarisation at the point in which the polarisation-vector meets it—that is, it is perpendicular to the ray.

We thus see brought out quite clearly the connection between Fresnel's theory and the electromagnetic theory for crystalline media.

The boundary conditions that must be satisfied at the passage between two crystalline media follow at once, if we assume that the transition takes place by a rapid but continuous change of the properties of the one medium into those of the other, and that the above equations hold within the region where this variation occurs. Taking the interface as the plane $x = 0$, we see that these conditions are the continuity of ϖ_2, ϖ_3, $\partial\Omega/\partial v$, $\partial\Omega/\partial w$; and since the curl of a vector has

no divergence anywhere, we may add to these the continuity of u and ϖ_1; but these conditions are clearly not independent of the former.

3. In the case of active crystals we are on less sure ground: we are without the guidance of Fresnel, who only considered the passage of light along the axis of an active uniaxal crystal, and our knowledge of the laws of propagation of light in such media is less definite.

It is, however, established that in any direction within an active uniaxal crystal there are two streams of permanent type that are oppositely polarised with their planes of maximum polarisation parallel and perpendicular respectively to the principal section, and Gouy* has shown that, neglecting small terms of the second order, the existence of these "privileged" streams may be accounted for by a superposition of the effects of ordinary double refraction and of an independent rotary power possessed by the medium.

If this be so, it is easily shown that to the same degree of approximation we have the following extension of Fresnel's theorem respecting the ellipsoid of polarisation:—

In any direction within an active crystalline medium two oppositely polarised streams can be propagated with their planes of maximum polarisation parallel respectively to the axes of the central section of the ellipsoid of polarisation parallel to the plane of the waves; and the propagational speeds of these waves are respectively in excess or defect of the speeds represented by the reciprocal of the length of either of these axes by an amount directly† proportional to the period of the vibrations of the polarisation-vector and to the ratio of the axes of the elliptic vibrations perpendicular and parallel to that axis of the section.

Thus if σ, σ' be the axes of the section, L, L′ the axes of the elliptic vibration parallel respectively to these axes, and ω_1, ω_2 the propagational speeds of the waves,

$$\omega_1 = \sigma^{-1} + \frac{\tau\rho}{4\pi}\frac{\mathrm{L}'}{\mathrm{L}} = \sigma'^{-1} + \frac{\tau\rho}{4\pi}\frac{\mathrm{L}}{\mathrm{L}'},$$

$$\omega_2 = \sigma^{-1} - \frac{\tau\rho}{4\pi}\frac{\mathrm{L}}{\mathrm{L}'} = \sigma'^{-1} - \frac{\tau\rho}{4\pi}\frac{\mathrm{L}'}{\mathrm{L}},$$

whence, approximately,

$$\sigma^{-2} = \omega_1^2 - \frac{\rho}{\kappa_1}\frac{\mathrm{L}'}{\mathrm{L}} = \omega_2^2 + \frac{\rho}{\kappa_2}\frac{\mathrm{L}}{\mathrm{L}'},$$

$$\sigma'^{-2} = \omega_1^2 - \frac{\rho}{\kappa_1}\frac{\mathrm{L}}{\mathrm{L}'} = \omega_2^2 + \frac{\rho}{\kappa_2}\frac{\mathrm{L}'}{\mathrm{L}},$$

where $\kappa = 2\pi/\lambda$.

* 'Journ. de Phys.,' (2), vol. 4, p. 142 (1885).

† By taking the excess or defect inversely, instead of directly, proportional to the period, a $(\partial/\partial t)^2$ is introduced in front of the rotary terms in (12).

Now the components of the polarisation-vector of a stream of elliptically polarised light may be represented by the real parts of

$$u = \bar{\alpha}\mathrm{D},\ v = \bar{\beta}\mathrm{D},\ w = \bar{\gamma}\mathrm{D},\ \mathrm{D} = \mathrm{A}\,.\,\exp.\,\{\iota\kappa\,(lx + my + nz - \omega t)\},$$

bars over the letters representing that they are complex, provided that the ratio $\bar{\alpha} : \bar{\beta} : \bar{\gamma}$ is not real, and if we so choose the origin of time that

$$\bar{\alpha}\mathrm{A} = \alpha\mathrm{L} + \iota\alpha'\mathrm{L}',\quad \bar{\beta}\mathrm{A} = \beta\mathrm{L} + \iota\beta'\mathrm{L}',\quad \bar{\gamma}\mathrm{A} = \gamma\mathrm{L} + \iota\gamma'\mathrm{L}',$$

then (α, β, γ), $(\alpha', \beta', \gamma')$ are the direction-cosines of the axes of the ellipse traced by the extremity of the polarisation-vector and L, L′ are the length of the axes in these directions.

Taking again equation (1) to represent the ellipsoid of polarisation, we obtain in place of equations (2) the two sets of equations

$$\left.\begin{aligned}
\left(a_{11} - \omega^2 + \frac{\rho}{\kappa}\frac{\mathrm{L}'}{\mathrm{L}}\right)\alpha + a_{12}\beta + a_{13}\gamma &= \mathrm{F}l \\
a_{12}\alpha + \left(a_{22} - \omega^2 + \frac{\rho}{\kappa}\frac{\mathrm{L}'}{\mathrm{L}}\right)\beta + a_{23}\gamma &= \mathrm{F}m \\
a_{13}\alpha + a_{23}\beta + \left(a_{33} - \omega^2 + \frac{\rho}{\kappa}\frac{\mathrm{L}'}{\mathrm{L}}\right)\gamma &= \mathrm{F}n
\end{aligned}\right\} \quad \ldots\ldots\ldots \quad (8),$$

and

$$\left.\begin{aligned}
\left(a_{11} - \omega^2 + \frac{\rho}{\kappa}\frac{\mathrm{L}}{\mathrm{L}'}\right)\alpha' + a_{12}\beta' + a_{13}\gamma' &= \mathrm{F}'l \\
a_{12}\alpha' + \left(a_{22} - \omega^2 + \frac{\rho}{\kappa}\frac{\mathrm{L}}{\mathrm{L}'}\right)\beta' + a_{23}\gamma' &= \mathrm{F}'m \\
a_{13}\alpha' + a_{23}\beta' + \left(a_{33} - \omega^2 + \frac{\rho}{\kappa}\frac{\mathrm{L}}{\mathrm{L}'}\right)\gamma' &= \mathrm{F}'n
\end{aligned}\right\} \quad \ldots\ldots\ldots \quad (9),$$

where F is given by (3) and F′ obtained from it by writing α', β', γ' for α, β, γ.

Whence we have

$$(a_{11} - \omega^2)\,\bar{\alpha} + a_{12}\bar{\beta} + a_{13}\bar{\gamma} = \bar{\mathrm{F}}l - (\mathrm{L}'\alpha + \iota\mathrm{L}\alpha')\,\rho/(\kappa\mathrm{A}) \ldots \quad (10),$$

and two similar equations, $\bar{\mathrm{F}}$ being obtained from F by writing $\bar{\alpha}$, $\bar{\beta}$, $\bar{\gamma}$ for α, β, γ.

Now (α, β, γ), $(\alpha', \beta', \gamma')$, (l, m, n) being the direction-cosines of three vectors at right-angles to one another, we have

$$\alpha' = \gamma m - \beta n, \qquad \alpha = -(\gamma' m - \beta' n),$$

and

$$\mathrm{L}'\alpha + \iota\mathrm{L}\alpha' = (\iota\mathrm{L}\gamma - \mathrm{L}'\gamma')\,m - (\iota\mathrm{L}\beta - \mathrm{L}'\beta')\,n = \iota\,(m\bar{\gamma} - n\bar{\beta})\mathrm{A},$$

whence (10) becomes

$$(a_{11} - \omega^2)\,\bar{\alpha} + a_{12}\bar{\beta} + a_{13}\bar{\gamma} = \bar{\mathrm{F}}l - \iota\,(m\bar{\gamma} - n\bar{\beta})\,\rho/\kappa \ldots\ldots\ldots \quad (11)$$

and two similar equations.

Hence, from the principle of interference expressed by

$$u = \Sigma\bar{\alpha}\mathrm{D}, \quad v = \Sigma\bar{\beta}\mathrm{D}, \quad w = \Sigma\bar{\gamma}\mathrm{D}, \quad \mathrm{D} = \mathrm{A}\exp.\{\iota\kappa(lx + my + nz - \omega t)\},$$

we obtain, as in §2,

$$(\ddot{u}, \ddot{v}, \ddot{w})$$

$$= \nabla^2\left(\frac{\partial}{\partial u}, \frac{\partial}{\partial v}, \frac{\partial}{\partial w}\right)\Omega - \left(\frac{\partial}{\partial x}, \frac{\partial}{\partial y}, \frac{\partial}{\partial z}\right)\left(\frac{\partial}{\partial x}\cdot\frac{\partial\Omega}{\partial u} + \frac{\partial}{\partial y}\cdot\frac{\partial\Omega}{\partial v} + \frac{\partial}{\partial z}\cdot\frac{\partial\Omega}{\partial w}\right)$$

$$-\rho\left(\frac{\partial w}{\partial y} - \frac{\partial v}{\partial z}, \frac{\partial u}{\partial z} - \frac{\partial w}{\partial x}, \frac{\partial v}{\partial x} - \frac{\partial u}{\partial y}\right) \quad \ldots\ldots\ldots \quad (12),$$

which may be written in the form

$$\dot{\mathrm{D}} = -\operatorname{curl}\varpi, \qquad \dot{\varpi} = \operatorname{curl}\mathrm{E} + \rho\mathrm{D},$$

D, E, and Ω having the same significance as in § 2.

The boundary conditions, obtained as in the former case, are the continuity of ϖ_2, ϖ_3, $\partial\Omega/\partial v$, $\partial\Omega/\partial w$, the interface being $x = 0$, together with the continuity of u and $\dot{\varpi}_1 - \rho u$, since within the transition-layer div. $\mathrm{D} = 0$, div. $(\dot{\varpi} - \rho\mathrm{D}) = 0$; the two latter conditions are not independent of the previous four, as

$$\dot{u} = -\left(\frac{\partial\varpi_3}{\partial y} - \frac{\partial\varpi_2}{\partial z}\right),$$

$$\dot{\varpi}_1 - \rho u = \frac{\partial}{\partial y}\cdot\frac{\partial\Omega}{\partial w} - \frac{\partial}{\partial z}\cdot\frac{\partial\Omega}{\partial v}.$$

4. When we come to the consideration of magnetically active media, our position is still more uncertain, but the following is suggested as an extension of Fresnel's theorem, being a generalisation of results that appear to be established for isotropic media.

In any direction within a magnetically active crystal two oppositely polarised streams can be propagated that have their planes of maximum polarisation parallel respectively to the axes of the central section of the ellipsoid of polarisation parallel to the plane of the waves: and the propagational speeds of these waves are respectively in excess or defect of the speed represented by the reciprocal of the length of either axis of the section by an amount that is inversely proportional to the period of the vibrations and directly proportional to the length of the axis, to the ratio of the axes of the elliptic vibration perpendicular and parallel to the axis, and to the component perpendicular to the section of a vector dependent upon the intensity of the magnetic field.

Thus if σ, σ' be the axes of the section, l, m, n the direction-cosines of its normal, b_1, b_2, b_3 the components of the vector B deter-

mined by the magnetic field, the propagational speeds ω_1, ω_2 of the waves are given by

$$\omega_1 = \sigma^{-1} + \frac{\pi}{\tau}\,\sigma\,(lb_1 + mb_2 + nb_3)\,\frac{L'}{L} = \sigma'^{-1} + \frac{\pi}{\tau}\,\sigma\,(lb_1 + mb_2 + nb_3)\,\frac{L}{L'},$$

$$\omega_2 = \sigma^{-1} - \frac{\pi}{\tau}\,\sigma\,(lb_1 + mb_2 + nb_3)\,\frac{L}{L'} = \sigma'^{-1} - \frac{\pi}{\tau}\,\sigma\,(lb_1 + mb_2 + nb_3)\,\frac{L'}{L}.$$

Whence, approximately,

$$\sigma^{-2} = \omega_1^2 - \frac{2\pi}{\tau}\,(lb_1 + mb_2 + nb_3)\,\frac{L'}{L} = \omega_2^2 + \frac{2\pi}{\tau}\,(lb_1 + mb_2 + nb_3)\,\frac{L}{L'},$$

$$\sigma'^{-2} = \omega_1^2 - \frac{2\pi}{\tau}\,(lb_1 + mb_2 + mb_3)\,\frac{L}{L'} = \omega_2^2 + \frac{2\pi}{\tau}\,(lb_1 + mb_2 + nb_3)\,\frac{L'}{L}.$$

Proceeding as in the last case we have instead of (11),

$$(a_{11} - \omega^2)\,\bar{\alpha} + a_{12}\bar{\beta} + a_{13}\bar{\gamma} = \bar{F}l - \iota\frac{2\pi}{\tau}\,(lb_1 + mb_2 + nb_3)\,(m\bar{\gamma} - n\bar{\beta}) \quad \ldots\ldots\ldots \quad (13),$$

and two similar equations; and applying the principle of interference we obtain the equations

$$(\ddot{u}, \ddot{v}, \ddot{w})$$
$$= \nabla^2\left(\frac{\partial}{\partial u}, \frac{\partial}{\partial v}, \frac{\partial}{\partial w}\right)\Omega - \left(\frac{\partial}{\partial x}, \frac{\partial}{\partial y}, \frac{\partial}{\partial z}\right)\left(\frac{\partial}{\partial x}\cdot\frac{\partial\Omega}{\partial u} + \frac{\partial}{\partial y}\cdot\frac{\partial\Omega}{\partial v} + \frac{\partial}{\partial z}\cdot\frac{\partial\Omega}{\partial w}\right)$$
$$-\left(b_1\frac{\partial}{\partial x} + b_2\frac{\partial}{\partial y} + b_3\frac{\partial}{\partial z}\right)\left(\frac{\partial\dot{w}}{\partial y} - \frac{\partial\dot{v}}{\partial z}, \frac{\partial\dot{u}}{\partial z} - \frac{\partial\dot{w}}{\partial x}, \frac{\partial\dot{v}}{\partial x} - \frac{\partial\dot{u}}{\partial y}\right) \ldots \quad (14),$$

which may be written in the form

$$\dot{D} = -\operatorname{curl}\varpi, \qquad \dot{\varpi} = \operatorname{curl} E + B\nabla\dot{D}.$$

The interface being the plane $x = 0$, the boundary conditions are the continuity of ϖ_2, ϖ_3, $\partial\Omega/\partial v + b_1\dot{w}$, $\partial\Omega/\partial w - b_1\dot{v}$, to which we may add the continuity of u and $\varpi_1 - b_2\partial u/\partial y - b_3\partial u/\partial z$, since within the transition-layer div. $D = 0$, div. $(\varpi - B\nabla D) = 0$: the number of independent conditions is, however, only four, as required for the treatment of magneto-optic reflection and the Kerr effect, since

$$\dot{u} = -\left(\frac{\partial\varpi_3}{\partial y} - \frac{\partial\varpi_2}{\partial z}\right)$$

$$\dot{\varpi}_1 - b_2\frac{\partial\dot{u}}{\partial y} - b_3\frac{\partial\dot{u}}{\partial z} = \frac{\partial}{\partial y}\left(\frac{\partial\Omega}{\partial w} - b_1 v\right) - \frac{\partial}{\partial z}\left(\frac{\partial\Omega}{\partial v} + b_1\dot{w}\right).$$

"On a Throw-testing Machine for Reversals of Mean Stress." By OSBORNE REYNOLDS, F.R.S., and J. H. SMITH, M.Sc., Whitworth Scholar, Victoria University. Received March 5, —Read March 20, 1902.

(Abstract.)

The present research, which was carried on in the Whitworth Engineering Laboratory of the Owens College, Manchester, was undertaken at the suggestion of Professor Osborne Reynolds, who proposed an investigation of "repeated stress" on the following lines:—The stress should be direct tension, and compression of approximately equal amounts, such tension and compression being obtained by means of the inertia force of an oscillatory weight. The rapidity of repetitions should be much higher than in the experiments of Wöhler, Spangenberg, Bauschinger, and Baker—in fact, ranging as high as 2000 reversals per minute.

In the apparatus employed a weight is supported vertically by means of the specimen to be tested, and the upper part of the specimen receives a periodic motion in a vertical direction by means of a crank and a connecting rod. The inertia of this weight gives a tension at the bottom end, and a compression at the top end of the stroke, the change from tension to compression being gradual. The specimen and parts are guided by suitable bearings placed in a vertical direction. The motion was made vertical in order to reduce the friction of the bearings to a minimum. The stresses can be changed by varying the diameter of the specimen, the load, and the speed of revolution of the crank. In order to enable one to calculate the stresses in the specimen, the centre of the crank shaft must be at rest, and the crank must move with uniform angular velocity. These conditions are obtained when the crank shaft is driven by a constant turning effort, if the moving parts of the machine are balanced, and if at the same time the total energy of the moving parts is invariable. The apparatus was therefore designed to satisfy these conditions as approximately as possible.

The apparatus was driven by the low-pressure engine of the triple expansion experimental engines, and had a speed indicator and a revolution counter attached. A great amount of trouble was experienced in lubricating the machine and in keeping the fluctuations of velocity small.

The specimens employed were carefully prepared, and with a few exceptions were of constant length and diameter. They were, in most cases, annealed before testing.

In conducting the tests the reversals for rupture were estimated

from the mean speed and the interval between the time of attaining full speed and breaking. In many cases the test could not be carried out without interruption, and for this reason the specimens had to rest, sometimes for a few days. The effect of these periods of rest on the total reversals for rupture was investigated, and found to be negligible.

In carrying out the tests it was found that for a series of tests in which the range of stress was being lowered (the specimens were of constant diameter), the limiting range of stress was more rapidly approached by this apparatus than by that of Wöhler. Since the diminution of range was obtained by diminishing the speed, this suggested that the limiting range of stress varied with the speed.

It is possible to use six different loads for the machine, and therefore to repeat a test with a given range of stress at six different speeds. It was observed that when more than one million reversals were required for rupture the rate of change of reversals with range of stress was very great indeed, and the author for this reason decided to limit in general the tests to one million reversals.

In the case of mild steel, six sets of tests were carried out corresponding to the six different loads which could be applied to the machine; six sets of results, similar to those of Wöhler, were obtained; these results were plotted, and the range of stress for rupture with one million reversals was obtained at six different speeds. The results were :—

Range of stress for rupture with 10^6 reversals.	Reversals per minute.
20·9	1337
20·1	1428
19·2	1516
18·1	1656
15·2	1744
12·4	1917

The mean result of statical tests for the mild steel employed in these experiments was :—

Yield stress	17·12 tons
Maximum stress	24·54 ,,
Breaking ,, ...	20·47 ,,
Percentage elongation.....	30

In the case of cast steel, four sets of tests were carried out. The results obtained were very little different from those of mild steel.

Thus the important conclusions arrived at are :—

1. The reversals for rupture with a given range of stress diminishes as the periodicity of the reversals increases.

2. The hard steels will not withstand a greater number of reversals of the same range of stress than the mild steels if the periodicity of the reversals is great.

"The Equilibrium of Rotating Liquid Cylinders." By J. H. Jeans, B.A., Isaac Newton Student and Fellow of Trinity College, Cambridge. Communicated by Professor G. H. Darwin, F.R.S. Received March 6,—Read March 20, 1902.

(Abstract.)

The most serious obstacle to progress in the problem of determining the equilibrium configurations of a rotating liquid lies in the difficulty of determining the potential of a mass of homogeneous matter of which the boundary is given. If this boundary is

$$f(x, y, z) = 0 \quad \text{(i)},$$

the potential will be a unique-valued function of x, y, and z, of which the form will depend solely upon the form of $f(x, y, z)$. This potential must therefore be deducible by some algebraical transformation of the function f.

In the method usually followed the solution is found as a volume integral, the integration extending throughout the surface (i). There is, however, a second method of obtaining this potential, namely, by regarding the potential-function as the solution of a differential equation, subject to certain boundary conditions. This leads directly to a series of algebraical processes, enabling us (theoretically) to deduce the potential by transformation of the function f.

In three-dimensional problems this method is quite impracticable, since it depends upon a continued application of the formula which expresses the products or powers of spherical harmonics as the sum of a series of harmonics.

As soon, however, as we pass to the consideration of two-dimensional problems, the spherical harmonics may be replaced by circular functions of a single variable. The transformation now becomes manageable, and for this reason the present paper deals only with two-dimensional problems, for which a method is developed enabling us to write down the potential by transformation of the equation of the boundary. The method is not of universal applicability, but is adequate to the problem in hand.

The method as applied to the determination of equilibrium configurations is as follows. Starting from the general equation (in polar co-ordinates)

$$r^2 = a_0 + 2a_1 r \cos\theta + 2a_2 r^2 \cos 2\theta + \ldots \quad \text{(ii)},$$

we transform by the substitution

$$\xi = re^{i\theta}, \qquad \eta = re^{-i\theta},$$

and attempt to solve the resulting equation explicitly for ξ in the form

$$\xi = b_1 + b_2\eta + b_3\eta^2 + \ldots + \frac{c_1}{\eta} + \frac{c_2}{\eta^2} + \frac{c_3}{\eta^3} + \ldots \ldots \ldots \text{(iii)}$$

this solution being such that the right hand gives the true value of ξ at every point of the surface given by equation (ii). The condition that the surface shall be an equilibrium surface under a rotation ω is found to be given by the system of equations

$$\frac{b_n}{n} = a_n\left(1 - \frac{\omega^2}{2\pi\rho}\right), \qquad (n = 1, 2, 3, \ldots).$$

The constancy of area of the curve (ii) can be effected by keeping c_1 constant. This method is subject to certain modifications, owing to the possibility of the various series becoming divergent.

The linear series of circles and ellipses (corresponding to the Maclaurin spheroids and Jacobian ellipsoids) are investigated without difficulty, and the points of bifurcation on these series are found. The first point of bifurcation on the latter series is shown to lead to a pear-shaped curve, similar to that of Poincaré, and it is shown that an exchange of stabilities takes place at this point.

The linear series of which this pear-shaped figure is the starting point can now be investigated, the equation being expanded in an ascending series of powers of a parameter θ. Since the equations are not linear, the calculation of terms multiplying high powers of θ is extremely laborious. The series is, therefore, calculated only as far as θ^5, this being found to give tolerable accuracy so far along the series as the expansion is required.

After passing through various pear-shaped configurations the fluid is found to assume a shape similar to that of a soda-water bottle with a somewhat rounded end. Beyond this the configuration is found to be suggestive of a tennis-racquet with a very short handle. A "neck" gradually forms at the point at which the handle joins the racquet, and this becomes more pronounced, until ultimately the curve separates into two parts.

As we proceed along this series the rotation steadily increases. At the point of bifurcation the value of $\omega^2/2\pi\rho$ is 0·375; when separation takes place this value is about 0·43. It is tolerably clear (although not rigorously proved) that when separation takes place, the primary may be regarded as the Jacobian ellipse, corresponding to rotation

$$\omega^2/2\pi\rho = 0{\cdot}43 \ldots \ldots \ldots \ldots \ldots \ldots \text{(iv)},$$

distorted by the tidal influence of the satellite. The linear diameters of primary and satellite are in a ratio of about 4 : 1.

The points of bifurcation on the Poincaré series are not investigated. Since the Jacobian ellipse determined by equation (iv) is known to be stable, there is ground for supposing that the series remains stable up to the point of separation. It therefore appears probable that the primary moves through a cycle of configurations in which Jacobi's and Poincaré's figures alternate. The angular momentum is decreased by about 30 per cent., at the ejection of each satellite.

"On the Action of the Spurge (*Euphorbia hiberna*, L.) on Salmonoid Fishes."* By H. M. Kyle, M.A., D.Sc., St. Andrews University. Communicated by Professor McIntosh, F.R.S. Received June 25,—Read December 12, 1901.

Introduction.

It has been known for some years that the Irish peasantry employed a simple method of procuring salmon and trout through the agency of the Spurge (*E. hiberna*, L.). The plant cut into small pieces and pounded with stones, or simply trampled upon at some convenient spot on a river, forms an emulsion in the water which, being swept downward into the pools, carries death to all fishes in its course. The fatality thus produced seems to have been enormous—80 to 100 salmon are reported to have been killed at one time,† and again in the Bandon rivers 500 to 1000 fish of various descriptions are said to have been poisoned during one season.‡ In the light of the experiments to be recorded presently, these statements do not seem exaggerated, for the Spurge-extract, even in small quantities, is almost as fatal to fishes as corrosive sublimate.

The fatal effect of the Spurge on fishes has been known in other countries besides Ireland, but to what ingredient or ingredients of the plant these effects are due seems never to have been investigated. The following pages contain a brief record of experiments which, though incomplete in many ways, throw considerable light upon the action of the Spurge, and open out to view some interesting problems.

As the range of this research has included within its scope several

* The Fishmongers' Company generously gave a sum for the carrying out of this research. Special thanks are also due to the Hon. G. W. Hely Hutchison, secretary to the Irish Inland Fisheries Commission, who forwarded plants of Spurge from Ireland.

† 'Report of the Inspectors of Irish Fisheries,' 1898, p. 193.

‡ *Ibid.*, 1892, p. 53.

branches of science, I have been obliged to seek assistance from specialists in the different departments, and I wish to acknowledge my indebtedness. Mr. Robertson, Lecturer in Botany at St. Andrews University, has aided me with the botanical literature on the subject, and the keen interest he took in the progress of the research was displayed in many valuable suggestions. To Dr. Fraser Harris, Lecturer in Physiology at the same University, I am even more indebted, since he freely gave his advice as to the best methods of conducting the physiological experiments which the research necessitated, and very kindly revised the manuscript. My indebtedness to the chemists will be displayed later, but I must here thank Professor McIntosh, F.R.S., for his kindly encouragement and the facilities he gave me for my work at the Gatty Marine Laboratory.

Description of the Plant and its Chemical Composition.

Euphorbia hiberna, L., or the Irish Spurge, is a member of the large order Euphorbiaceæ, which gives to medicine such well-known drugs and poisons as croton oil, cascarilla, and castor oil. It is a hardy perennial weed, growing to the height of about 2 feet, with dull greenish leaves and inflorescences of an orange-yellow colour. Its area of distribution is mostly on the Continent in France, Switzerland, and Italy, but it is also found in the South of England and South of Ireland. Its reputation is well known, for in all reference works, botanical or pharmacological, where it is mentioned, one finds the statement that it is used in Ireland to poison fishes.

The fresh plant is almost odourless, but the expressed juice from the laticiferous vessels and the emulsion formed in water have a very faint, pungent, and slightly aromatic odour. In this respect it differs somewhat from the officinal Euphorbia (*E. resinifera*, Berg), a native of Northern Africa, which is said to have a powerfully irritating acrid odour. In the latter, according to Flückiger,* who examined the dried exported drug, this acridity is due to amorphous indifferent resin. It is probable that the fresh extract—with which alone we are concerned in *E. hiberna*—would differ somewhat from the analysis given by him. Thus he found no volatile oil on distilling the drug with water, whereas a small quantity of some volatile substance was obtained from *E. hiberna* by the same means. Again, he declares that no emulsion is formed in water by the drug obtained from *E. resinifera*. On the other hand, an emulsion characterises the fresh aqueous extract of *E. hiberna*. Flückiger's analysis would seem, therefore, to apply to the oxidation-products of the original substances in *E. resinifera*, and is hardly comparable with that of *E. hiberna* to be presently given.

In one respect *E. resinifera* differs essentially from *E. hiberna* : in the

* 'Pharmacognosie des Pflanzenreiches,' p. 194; 'Pharmacographia,' p. 504.

analysis of the former given by Flückiger no mention is made of tannic acid, which in the latter is present in considerable quantity. This constituent has, however, been found in other species of Euphorbiaceæ—*e.g.*, *E. lathyris*, L.,* a native of Southern Europe but naturalised in England and Scotland. The following preliminary chemical examination of the Spurge was made by G. D. Lander, D.Sc., late of St. Andrews University :—

"The root-bark and stem contain a milky latex of acid reaction. The root-bark is particularly rich in latex. On standing, the expressed latex speedily solidifies to a gum-resin.

"Cold water forms a light-brown emulsion of characteristic odour. On boiling the bark with water, a more concentrated extract, darker in colour, may be got. A small quantity of a solid volatile with steam was separated. This appears to be the odoriferous body. The liquid condensed from the boiling water is neutral, indicating the absence of volatile organic acids or alkaloids. No indication of the presence of alkaloids could be got in a hydrochloric acid extract. On distilling with alkali, an alkaline distillate is obtained, but this is probably due to ammonia.

"The liquid obtained by extraction of the root-bark with hot water was an acid emulsion of resin, in a solution containing starch, and probably also dextrin and gum. Much resin is left unemulsified in the bark when the extraction is effected by hot water.

"The concentrated extract gave a copious precipitate when mixed with 10 volumes of absolute alcohol. This precipitate indicates the presence in the extract of dissolved starch, and possibly also dextrin and gum, which are probably innocuous. The alcoholic solution contains resin associated with some gum. The water extract of stem and leaves possessed the same properties."

Inasmuch as it was found by experiments on trout that the volatile substance could not account for the fatal action of the Spurge, but that the acid emulsion of resin did, Dr. Lander prepared a quantity of this by the alcoholic method, carefully removing the alcohol by evaporation after the emulsion had been formed. This emulsion so prepared had precisely the same fatal effects on trout as the freshly prepared aqueous extract of the Spurge.

Indications were obtained, however, at a later stage of the work, that the Spurge-extract might still further be resolved into its components, and the following examination was thereupon carried out at the physiological laboratory.

The emulsion formed by cold water is of a dirty-grey or light-brown colour, with a small quantity of a powdery solid in suspension. By repeated filtration this emulsion is broken up, and a clear brownish solution is obtained. If the emulsion is retained in a tightly stop-

* Hooker, 'Flora' (1870), p. 329.

pered bottle, it remains unchanged for many days, but if left open to the air even for a few hours, rapid changes take place. A brownish flocculent precipitate gradually takes the place of the greyish emulsion, and remains suspended for some time in the solution, which is now becoming of a dark brown colour. If this solution is filtered, and the filtrate again allowed to stand in the air, a precipitate again forms similar to the previous one, and this process may go on for weeks.

It is obviously impossible to perform satisfactory tests upon the original emulsion, and on the other hand examination of the filtrates cannot be expected to give exact information with regard to the original extract. Such an examination seemed necessary, however, because the results obtained would throw light upon the changes the extract had undergone, and reasoning from these results, the alterations which the Spurge-extract undergoes when thrown into the rivers in Ireland, might be followed.

A minute quantity of proteid material was detected by means of the xanthoproteic, Millon's, and other tests. The presence of this proteid interfered with the reactions given by other constituents of the extract, but otherwise it is of no importance. Starch granules were readily identified by means of iodine in the precipitate which forms in the extract on standing. Glucose was identified by means of the phenylhydrazin test, and was present in considerable quantity.

Tannin, or some ally of tannic acid, was identified by the precipitates given by the salts of iron and copper, as well as by various other reagents. Solutions of tannic and gallic acids were then prepared in varying proportions, and it was found that solutions of approximately 1 per cent. gave reactions similar (as to colour, quantity, and rapidity of appearance of the precipitates) to those given by the aqueous Spurge-extract. As the strength of the extract was about 1 gramme of plant to 1 c.c. of water, it follows that the plant contains at least 1 per cent. of tannic acid. The percentage is probably greater than this, because much tannin remains in the tissues of the plant after the first filtrate is removed, and also because the presence of the proteid material inhibits somewhat the reactions given by tannic acid.

The constituents of the aqueous Spurge-extract would thus seem to be: 1, proteid; 2, starch; 3, glucose; 4, resin and gum; 5, tannic acid; 6, volatile substance.

The metals calcium, sodium, and potassium are also present, but in what form was not determined. It is worthy of special notice that no alkaloid was found in the latex. If the analysis just given be compared with those worked out by botanists for other species of the Euphorbiaceæ, it will be found that *E. lathyris** comes nearest to *E. hiberna.*

* De Bary, 'Comparative Anatomy of the Phanerogams and Ferns,' English translation, 1884, p. 184.

In *E. lathyris* as well as in other species of the Euphorbiaceæ, malic acid in the form of its calcium and other salts occurs in abundance, and it is possibly present in *E. hiberna*. If present in the acid form in the extract, it would account for some of the changes that occur when the extract is exposed to air, but otherwise it is of little importance.

In his description of *E. resinifera*, Flückiger mentions the presence of a compound which he calls Euphorbon, with the composition $C_{15}H_{24}O$ or $C_{20}H_{36}O$. Whether this substance, probably an oxidation product, or any similar substance, is present in *E. hiberna* was not determined, and it is due in part to the incompleteness of the chemical analysis of the plant, that certain discrepancies to be shown hereafter can not be fully explained.

It is necessary to mention the various extracts of the Spurge that were employed, and the methods by which they were prepared. What will be referred to as the "fresh extract" means an aqueous extract which has been prepared by washing and chopping up the stems, roots, and leaves of the plant, and allowing them to stand in water within a closed vessel. The vessel was shaken well several times, and at the end of 2 hours the liquid was poured off, filtered once, and measured. The aim of this method was to obtain an extract resembling as nearly as possible that which is employed in Ireland in poisoning the rivers, and was decided upon after various trials. It was found that if more than 2 hours were given to the formation of the extract the emulsion tended to break up and disappear. If the chopped-up plant was left for 24 hours in water the emulsion still persisted, but was not so thick and disappeared on once filtering, whereas on standing for 2 hours only, the emulsion passed through the filter and remained in the filtrate. The reason for filtering at all was to get rid of a large amount of woody and insoluble material which could play little or no part in the action of the Spurge on fishes, but made the liquid turbid and obscure.

Whether the emulsion was present or absent, and whether the liquid was poured off from the plant at the end of $\frac{1}{2}$ hour or 4 days, made no difference in the fatal effects which it had upon the trout. But it was found desirable to have some standard solution with which all other extracts and preparations could be compared. The standard was arrived at by comparing the amount of the liquid drawn off with the weight of the plant employed—1 c.c. of liquid to 1 gramme of plant being taken as the 100 per cent. extract. When percentages of the fresh extract are spoken of, therefore, they are calculated from this as the standard.

This is admittedly a rough measure and for two reasons. On the one hand, it does not express the percentages in terms of the active ingredient, which would have been very desirable indeed had it been possible. On the other hand, the short space of 2 hours during which the plant is macerated, does not by any means exhaust the active

ingredients of the plant. The process of maceration may be repeated on the same plant three or four times, and the resulting liquids will still be as fatal or almost as fatal in their action on the trout as the first. This, although it does not do away with the value of the standard chosen, shows that the active ingredient or ingredients are present in the plant in larger quantity than is represented by the standard fresh extract. Further it shows that the deadliness of the plant, when placed in the Irish rivers, is not a matter of a few minutes only, but of many hours and even days.

The fresh extract prepared as described has all the characteristics which have been already given. In addition, however, it is highly oxidisable, as its power of rapidly decolourising potassium permanganate shows. It should be mentioned also that the composition of the plant may vary throughout the year, and that the above examinations were made in May and June. The changes which the extract undergoes on exposure to air, as well as the initial composition of the plant and its change during the year, demand more thorough determination ere a complete explanation can be given of all the effects the plant-extract produces. Here it is only possible to account for a few, but perhaps the most important phenomena.

Action of the Fresh Spurge-extract and Latex.

When rubbed on the skin, no sensation is noticeable, but the surface becomes dry and rough. Later, the part becomes slightly flushed. When taken by the mouth in small quantities it has a nauseous, disagreeable taste. At the back of the tongue and in the gullet it produces an irritant, burning sensation, which remains for several hours. Salivation, if any, is very slight. In the stomach it has little effect. At first there is a slight tendency to nausea, but this soon passes away and no ill-effects of any kind ensue.

The solution which has stood for some time and been repeatedly filtered, loses this effect. It is still slightly acid, and leaves a soft, soapy sensation on the back of the tongue, whilst in the stomach it has no effect whatsoever.

It is said that the salmon which are killed by the Spurge-extract in the Irish rivers are used as food without any ill-effects resulting. In order to test this point, a stray kitten which had wandered into the Gatty Laboratory was permitted to feed upon the poisoned trout. When no signs of poisoning were shown, the emulsion obtained from the extract after removal of the volatile substance, was placed in its milk—about a quarter being emulsion. This diet was continued for 4 days, at the end of which time the kitten appeared in good condition and health, and its appetite for the poisoned trout and milk had in no way diminished. But not only has the Spurge-extract little

or no effect on the intestinal canal of the higher animals, it has also none on that of the fishes, as the following experiment showed. Several trout were set apart in a separate tank by themselves and fed for 6 weeks on boiled mussels, which had been steeped for some time in the Spurge-extract. The extract employed was the ordinary aqueous preparation, containing therefore a certain amount of the volatile substance, and fatal to the trout when immersed bodily into a small proportion of it in water. Yet when taken into the alimentary canal, through the mussel as medium, it had no apparent effect—the trout living upon the food appeared as active and as healthy at the end of 6 weeks as those in the neighbouring tank, which had been fed upon the ordinary boiled mussel. There could be no doubt that the fish really absorbed some of the extract, because the mussels were so thoroughly steeped in the extract that some of it must have remained unchanged in their tissues; and again the trout were kept in a half-starved condition, so that when fed they eagerly grabbed the mussel before it had been more than a second or two in the water. There was no time therefore for the extract to diffuse out into the water, and it must consequently have been ingested.

Not only does the aqueous extract of the Spurge—with which we are mostly concerned—seem to have no action on the alimentary canal in moderate quantities, it also seems incapable of being absorbed from the skin, or if absorbed is not fatal. Several toads which were immersed for 7 days in an aqueous extract, but so that their heads and necks were above the surface and the animals were thus able to breathe freely, were alive at the end of that time and able to respond to stimuli. This extract had been freshly prepared, and produced certain characteristic effects on the vascular system to be presently described. What happened in the case of the toads can readily be inferred from the known action of tannic acid. The peripheral circulation would be affected, and the skin might indeed be "tanned" to a certain extent, but this would protect the internal organs from harm, and evidently did so whilst the toads were immersed in the solution.

From the foregoing series of observations, it might be inferred that the action of the aqueous Spurge-extract is mainly local, and the following experiments seem to decide conclusively in favour of this inference. The gastrocnemius muscle of pithed frogs gave the normal responses to stimuli, both when irrigated directly by the extract and when the latter had been previously injected into the lymph. The heart of a pithed toad when irrigated by the extract, as well as after injection into the lymph, varied as little from its normal beating as it did when irrigated by normal saline solution. These experiments were performed both with the fresh extract and with the extract some days old, and it may clearly be inferred that the aqueous Spurge-

extract has no action on muscular tissue when treated in the above way. Injection of the extract into the muscle was not attempted.

Although both the motor nervous and muscular systems are not affected, the same cannot be said for the sensory nervous system. Very few experiments were made on this point, but they seem sufficient to show that the sensory nerve-tracts are slightly paralysed at some part of their course. The reflex time of two pithed frogs, which were allowed to remain in this condition for some hours after the operation to allow them to recover from the shock, was found to be several seconds greater on the average after injection of the Spurge-extract than before. And again, two similarly treated frogs responded to weaker electrical stimuli before injection of the extract than after. Taken in conjunction with the previous experiments on the motor system, these results show that the sensory nerves or central nervous system are in some way affected.

The quantity of volatile substance which is present in the fresh aqueous extract, though it produces a slight paralysis of sensation, is not sufficient to account for the fatal effects which the same extract has on fishes, and also on frogs when treated in a certain manner.

If a pithed frog's lung be dissected out and irrigated with normal saline solution, the circulation in the capillaries may be watched for a long time. When now a few drops of the Spurge-extract are placed on the lung, a great and almost immediate change is seen. The capillaries shrink visibly, the blood corpuscles are pressed backwards into the arterioles and sway to and fro with each beat of the heart. The heart-beats become slower and more protracted if the Spurge irrigation is continued. The whole lung contracts to half its previous size, and in the larger vessels which also contract, a condition of stasis sets in, ending with the death of the frog within half an hour of the operation. If the mesentery, and not the lung, be chosen for examination, a similar result is obtained, but this has the additional advantage of showing that the action is quite local. If a small portion of the intestine is irrigated with the extract, the blood ceases to flow in the arteries leading to that part, but not in the neighbouring ones.

The effects here described are obviously due to the tannic acid, and the application of the latter directly produces the same result. Even a weak solution, 0·01 per cent. of tannic acid (1 in 10,000), has a marked effect upon the capillaries, whilst a 1 per cent. causes stasis almost immediately. Further, when the lung is irrigated with normal saline solution after treatment with 0·01 per cent. of tannic acid, circulation is gradually re-established, but not so after 1 per cent. The latter case resembles that of the Spurge-extract, for irrigation with normal saline solution after treatment with the fresh extract enables the circulation to be maintained in the vessels which had not become constricted, but cannot revive it in the others.

Table I.—Summary of Experiments which were made in order to discover the Action of the Spurge-extract on Fishes and other Animals.

Strength and nature of solution.	Period within which it is fatal.	Animal experimented on.	Mode of application.	Notes.
20 per cent. of "fresh extract" (=0·2 per cent. of tannin approx.)	5 minutes	Trout	Animals immersed in solution	Control experiments performed at same time. Other trout immersed in fresh water in vessel of similar form. The variation in length of period which proved fatal was considerable.
5 per cent. of do.	10 to 30 minutes	Do.	Do.	This probably arose from the condition of the animals when experimented on.
4 per cent. of do.	10 to 30 minutes	Do.	Do.	The symptoms were the same in both strong and weak solutions. In 50 per cent. of cases great excitement shown—the fish darting about wildly for some time, then resting exhausted, frequently in an inverted position. Rate of breathing increased from 120 to 130 per minute—the rate in control specimen—to 140 to 160 per minute. In 50 per cent. of cases no excitement, and animal remained in normal position. Gulping movement of jaws at intervals of 5 to 10 respiratory movements, accompanied by jerking movements of body. Later, rate of breathing became gradually slower.
1 per cent. of do.	¾ and 1½ hours	Do.	Do.	
0·2 per cent. of do.	1 to 2 hours	Do.	Do.	
0·1 per cent. of do.	1½ to 2 hours	Do.	Do.	
0·1 per cent. of do. after standing for 6 days	2 to 4 hours	Do.	Do.	
0·05 per cent. of do.	4 to 6 hours	Do.	Do.	Loss of sensibility then displayed. Gill-covers held apart from pectoral arch so that gills could be seen during inspiration.
0·01 per cent. of do.	4 to 6 hours	Do.	Do.	
0·2 per cent. of "fresh extract" formed from fruit and leaves	2½ to 4 hours	Do.	Do.	Roots seem to have more of poisonous ingredients, but these experiments were performed early in September, when stem and leaves begin to lose their sap.

Strength and nature of solution.	Period within which it is fatal.	Animal experimented on.	Mode of application.	Notes.
0·2 per cent. of "fresh extract" from stem and leaves	2 to 4 hours	Trout	Animals immersed in solution	
0·2 per cent. of "fresh extract" from roots	1 to 3 hours	Do.	Do.	
0·2 per cent. of emulsion minus volatile solid	40 minutes to 1 hour	Do.	Do.	The emulsion prepared in this way is stronger than the fresh extract. Thus 0·2 per cent. of this is equivalent to about 0·5 per cent. of fresh extract.
0·2 per cent. of emulsion minus volatile solid. 4 weeks later	1 to 2 hours	Do.	Do.	
Solution of volatile solid		Do.	Do.	Death did not result, although animals displayed excitement. Perhaps quantity too small.
1 per cent. of "fresh extract" of Spurge		Gastrocnemius of pithed frogs	Direct irrigation	No effect produced. The responses given did not differ from those of normal frog.
100 per cent. of do.		Do.	Do.	
100 per cent. of do.		Do.	Injected into lymph space	
100 per cent. of do.		Heart of pithed toads	Direct irrigation, and injection	No effect produced. Heart's action remained normal.
100 per cent. of do.	..	Lung of pithed frog	Direct irrigation	The lung contracted visibly. The capillaries were constricted and a condition of stasis set up in larger vessels, all within a few minutes.
20 per cent. of do.	..	Do.	Do.	
0·1 per cent. of tannic acid	..	Do.	Do.	The effect produced was precisely similar to that described as due to the Spurge-extract, the stronger solutions being more rapid in their action.
1 per cent. of do.	..	Do.	Do.	

Strength and nature of solution.	Period within which it is fatal.	Animal experimented on.	Mode of application.	Notes.
100 per cent. of "fresh extract."	..	Pithed frog's mesentery	Direct irrigation	Effect as on lung, but seen to be local. Circulation was maintained in neighbouring vessels not affected by the extract.
20 per cent. of do.	..	Do.		
2 per cent. of do. (approx.)		Frog	Immersed in solution, but so that head was free	Lived thus for 7 days.
Do.	..	Frog pithed	Injection	Reflex time for response to stimuli taken before and after injection, greater in latter case.
2 per cent. of do. (approx.)	..	Frog pithed	Injection	A stronger electrical stimulus required to obtain a response, under the influence of the Spurge-extract than under normal conditions.
0·01 per cent. tannic acid	5 to 6 hours	Trout	Immersed in solution	Fish quiescent, no paralysis apparent, breathing irregular after some time, gill-covers a little strained but not greatly.
0·02 per cent. tannic acid	2½ hours	Do.	Do.	*Post-mortem* examination showed that great sloughing of the epithelium of the gills had taken place, more especially in the stronger solution.
Finely powdered starch in state of suspension in water	..	Do.	Do.	Animal immersed for 5 hours without showing any signs of discomfort. Animal took food in solution. A fine powder, therefore, is not harmful in itself.
Emulsion of resin and mucilage in water	..	Do.	Do.	No effect.
0·01 per cent. tannic acid and starch and aleurone	2½ hours	Do.	Do.	Rapidity of action apparently increased.
0·5 c.c. of "fresh extract"	12 hours	Do.	Injected into abdomen	The first specimen injected was much larger than other two — the third small and thin. Symptoms similar. At first no signs of change.

Strength and nature of solution.	Period within which it is fatal.	Animal experimented on.	Mode of application.	Notes.
0·4 c.c. of do.	6 hours	Trout	Injected into abdomen	After 2 hours diminished sensibility shown, but this passed off later.
0·3 c.c. of do.	3½ hours	Do.	Do.	Breathing heavy and deep with gulping movements of jaws intermittently. In 2nd and 3rd colour became very dark, with the red spots more conspicuous; for *post-mortem* examination, *vide* p. 63.
10 per cent. of do.	2 hours	Trout post-larval	Animal immersed in solution	Weak solutions of Spurge and tannin had little apparent effect upon early stages of trout. The circulation was hardly affected. In later stages stasis was shown in the capillaries at tip of notochord and in pseudobranch of gill-cover. Over yolk-sac and body, however, circulation maintained. This both in Spurge solution and in tannic acid.
10 per cent. of do.	1 hour	Do. later stage	Do.	
0·01 per cent. tannic acid		Do.	Do.	
0·1 per cent. tannic acid	1 hour	Trout, post-larval (later stage)	Animal immersed in solution	Heart-beats increased from 50 to 60 when at rest and 80 to 85 when active to 90 to 110 per minute. Later frequency fell to 80, and then became gradually slower until death occurred. Gulping movements of the jaws frequent, synchronous, or almost so, with failure of ventricular beat. Heart continued to beat feebly for some minutes after circulation in body had ceased.

If we turn now to the action of the Spurge on fishes, we shall find that the theory which has accounted for its action on frogs renders it possible to understand a series of complex phenomena, which at first sight are very puzzling. Fishes, unlike frogs, are extremely "nervous" animals, and they display symptoms of fright or excitement, which must be carefully distinguished from those really due to the Spurge. The details of the experiments are given in the annexed

table. Here it is only necessary to give analysis of the symptoms and explain how they are caused.

One of the symptoms frequently displayed by the trout after immersion in a solution of the Spurge-extract is great excitement. The animals dash wildly about, and soon or late turn over on their backs, remaining thus for a long time if the solution is weak, until they die. Of twenty examples, ten displayed these phenomena, but the other ten showed no excitement, and remained in the normal position until within a few minutes of death. This variability was seen both in weak and strong solutions of the extract, and makes it impossible to believe that these symptoms are directly due to the Spurge. This negative conclusion is further strengthened by the fact that those fishes which possess air-bladders, and especially the trout, very readily lose their balance on seemingly slight provocation. Thus, if these trout are placed in water of higher temperature than they have been accustomed to, or even if exposed in a glass vessel to the rays of the sun—many of them will turn over on their backs and remain so until placed in colder water, or until they die. Sometimes, also, if suddenly startled, they display all the symptoms of excitement described above; and the only conclusion which can be drawn is, that the presence of anything unusual if sufficiently powerful, will induce a state of excitement in the trout leading to a loss, temporary or permanent, of the power of balancing themselves.

This loss of balance seems to arise from an affection of the central nervous system, causing an increased secretion of the gaseous contents of the air-bladder, at the same time having an inhibitory effect on the muscular tissue surrounding the opening of the air-bladder into the œsophagus, because it is invariably noticed in those trout which have a period of excitement and then come to rest, exhausted, yet remaining in the normal position, that successive bubbles of gas escape from their mouths, whereas no bubbles are to be seen in those cases where the trout loses its balance. Why this nervous affection should occur in some cases and not in others is as difficult to explain as the origin and variability of nervous diseases in general.

Another symptom of the same order is a peculiar gasping movement or spasm of the jaws and gill-covers, which is of constant occurrence and increases in frequency the longer the trout are immersed in the solution of the Spurge. But this phenomenon may appear under a variety of circumstances. Professor McIntosh noticed it whilst experimenting with various drugs upon young salmon, and he found that each spasm was accompanied by a discontinuity in the contractions of the ventricle. It may at times be noticed in the trout when under normal conditions, but occurs more frequently under abnormal conditions—as, for example, after the trout have been startled and excited in any way. It thus may be one of the

results of the nervous affection mentioned in the previous paragraph. Whilst these spasms therefore arise from the presence of the Spurge and may be called a secondary effect, they cannot be considered as characteristic of nor as directly due to its action.

Another symptom which observation, if confined to the action of the Spurge on trout, might lead one to think diagnostic, is the constant shedding of mucus from the mouth and skin. And one is the more ready to admit this, seeing that tannic acid, which has this effect, plays an important part in the action of the Spurge. But if trout are placed in clean water in a glass vessel immediately on capture, it will be noticed, after they have calmed down from their excited condition, that the water contains an abundant quantity of stringy mucous material. The shedding of mucus seems to occur very readily in salmonoid fishes, perhaps in all, and may be a symptom of the nervous excitement into which the animals are thrown. Again, the change that comes over the breathing may be due to the same cause. The frequency of this is increased during the first few minutes, but later falls below what it was to begin with. In the young post-larval trout, it was noticed that this occurred synchronously with a rise and fall of the pulse-frequency.

Only on two occasions was any sign given that the digestive system was interfered with. In these cases the trout, after being immersed for some time in a solution of the Spurge, vomited the contents of their stomachs. This again seems to arise from the derangement of the central nervous system already mentioned.

Excluding these cases there are still one or two symptoms which seem to display the direct local and distant effects of the Spurge, and though separately each may arise from other causes, yet taken together they seem to be characteristic of the action of the Spurge.

When the trout have been immersed for some time in the solution of the Spurge, it is noticeable that the gill-covers do not close completely over the gills, and that the latter can partly be seen even when the former should be quite closed. Further, the gills themselves appear swollen and of a brighter scarlet than under normal conditions. *Post-mortem* examination did not show any great difference from the normal. Sometimes a portion of the gill filaments would be injected with blood, showing that inflammation had been present there, but usually the naked-eye appearance was quite normal. In sections the poisoned specimens showed considerable disintegration of the mucus epithelium of the gills, rupture having apparently taken place in the underlying connective tissue, so that the capillaries which remained intact were left denuded. This was not present everywhere, but was nevertheless distinct from the normal condition. The capillaries also were much contracted and of smaller calibre than is usually the case. Further, though this difference from the normal

was not so distinct as the others, the larger blood-vessels were distended with red blood corpuscles.

Another symptom which is very apt to be overlooked, but which seems undoubtedly to be due to the Spurge, is a distinct loss of sensibility displayed by the trout after being immersed for some time in the solution of the extract. They become quiescent and sluggish, and whether lying on their backs or in the normal position permit themselves to be touched and moved about by the finger. This paralysis does not extend however to the motor system, because they are able to dart about in their accustomed manner if a sufficiently strong stimulus be given. Only two cases were noticed out of twenty in which the body was contorted and there seemed to be motor paralysis of one side. As death approached this sensory paralysis increased and it required still stronger stimuli to rouse the animal, but except in the two cases mentioned the motor system seemed fully capable of performing its functions until within a few minutes of death. If the solution of the Spurge is strong, death ensues so rapidly after immersion that it is somewhat difficult to detect these various stages, but they are easily followed when death does not take place for an hour or more, as in 0·1 per cent. solutions and under. This loss of sensibility is evidently similar to what occurs in the frogs, but in the trout it is more conspicuous.

Neither macroscopic nor microscopic investigation (the latter by sections) revealed any change in the tissues which could account for the fatal action of the Spurge. The alimentary canal (excluding the gills), the liver, spleen, and kidneys presented precisely the same appearances in the normal trout and in those killed by immersion in solutions of the Spurge. When the Spurge-extract was injected into the abdominal cavity, however, a distinct change was noticed in the vascular system of the alimentary canal. The methods employed for fixing and staining the tissues* were the same in all cases, so that the change to be described could not arise in this way. The capillaries lying in the submucosa within the circular muscular layer of the stomach and intestines were found to be dilated and filled with red corpuscles, showing that congestion had occurred. In the normal trout, and in those killed by immersion bodily in the Spurge-extract, these blood-vessels appeared in section as small loose-walled capillaries containing but few corpuscles and often none at all. This change had evidently been produced by the tannic acid component of the Spurge, in the manner already mentioned when describing the action of the Spurge and tannic acid on the vascular system of the frog. The blood-

* Fixation, by mixture containing gl. acetic acid (7 parts), 40 per cent. formaldehyde (3 parts), 70 per cent. alcohol (90 parts). This was found to give better results for epithelium than either corrosive sublimate or Flemming's solution. Staining, most usually by hæmalum and alcoholic eosin.

vessels of the kidney did not display this difference from the normal condition, which lends further proof of the quite local action of the component of the Spurge which has fatal effects, because the kidneys lie behind the air-bladder and are thus protected from whatsoever may be injected into the abdominal cavity, unless it reaches them by way of the circulation.

The time taken by the Spurge-extract to be fatal when injected into the abdominal cavity may be quoted here in order to show that the fatal effects of the Spurge in rivers arise from some action on the gills. An injection of 0·5 c.c. of fresh extract took more than 12 hours to be fatal. The same quantity in 500 c.c. of water would be fatal within less than 2 hours were the fish immersed in it, that is, if it acted on the gills. A stoppage in the gills affects the whole circulation, whereas in any other part the effect is local.

The experiments recorded in Table II give some notion of the deadly nature of the Spurge. Only in four cases out of many did the fish recover when removed from the Spurge solution to fresh water. In the first two cases the animals were immersed in strong solutions for a short time, too short apparently for the Spurge to have fatal effects, although the inertness of the fish when removed to fresh water showed that the poison had really affected their tissues. The other two cases show that the extract becomes somewhat less deadly after standing for several days, probably because a fungus gradually develops in it and thereby lessens the amount of tannic acid in the solution.

When the fish are immersed in solutions of pure tannic acid, the effects are similar to those produced by the Spurge-extract. The solutions employed were of 0·01 per cent. and 0·02 per cent. strength. In the former, death occurred within 4 hours, in the latter within 2 hours. There was no excitement on immersion, no loss of sensation, nor loss of balance. The gill-covers displayed spasmodic movements and were maintained wider open than in the normal trout, just as when the fish were under the action of the Spurge-extract. Also, when the trout were removed from the solution of tannic acid to fresh water they failed to recover. *Post-mortem* examination showed that the sloughing of the epithelial covering of the gills was more marked in the case of tannic acid, especially in a 1 per cent. solution, than in the case of the Spurge. A further difference was the absence of any sign that the nervous system was affected.

A comparison of the times which the tannic-acid solutions and the Spurge-extract solutions took to be fatal, given in Table I, shows that the latter is more rapidly fatal in its action than the supposed percentage of tannic acid it contains (1 to 2 per cent.) would allow one to expect. A 0·1 per cent. solution of the fresh extract of the Spurge is fatal in less than two hours, and this solution, according to the calcu-

lation, should contain about 0·001 per cent. of tannic acid. This latter quantity of tannic acid should therefore be fatal within 2 hours, whereas a solution ten times stronger took 4 hours, and one twenty times stronger just the same time, viz., 2 hours. After making due

Table II.—Experiments to Test the Power of Recovery which the Trout possessed after being immersed in various solutions for some time.

Strength and nature of solution.	Period within which it is fatal.	Duration of immersion.	Recovery, or lapse of time until death occurred.
20 per cent. of "fresh extract" (0·2 per cent. tannic acid, approx.)	5 minutes ..	3 minutes	Recovery.
5 per cent. of "fresh extract"	10 to 30 minutes	3 minutes	Recovery.
5 per cent. do.....	10 to 30 minutes	15 minutes	Death within 20 minutes.
1 per cent. do.....	1 to 1½ hours	1 hour ..	Death within ½ hour.
0·1 per cent. do., 4 days old	1½ to 2 hours	1¼ hours	Death within ½ hour.
0·1 per cent. do., 5 days old	1½ to 2 hours	1¼ hours	Death within ½ hour.
0·1 per cent. do., 6 days old	1½ to 4 hours	1¼ hours	Death within 1¼ hours.
0·1 per cent. do., 8 days old	1½ to 4 hours	¾ hour ..	Death within 3 hours.
0·1 per cent. do., 10 days old, and from which a precipitate had been filtered	..	2 hours ..	Recovery.
0·01 per cent. do.	4 to 6 hours	4 hours ..	Death within 10 minutes.
0·01 per cent. do.	4 to 6 hours	3½ hours	Death within 1½ hours.
0·01 per cent. of "fresh extract." 2 days old	..	4½ hours	Death within 1½ hours. One specimen recovered.
0·2 per cent. of emulsion minus volatile solid. (= 0·5 per cent. "fresh extract" approx.)	¾ hour	40 minutes	Death within 10 minutes.
As do., but 4 weeks later	1½ hours	1 hour	Death within ¾ hour.
0·01 per cent. of tannic acid	5 to 6 hours	4½ hours	Died 1 hour later.
200 per cent. of tannic acid.	2½ hours	2 hours	Died within 1 hour.

allowance for variability in specimens, it seems certain from this that the quantity of tannic acid, roughly estimated to be in the Spurge-extract, is much less than the real quantity, or it may be that the other components of the extract play a more important part than has been credited to them.

Some mention should be made of the peculiar phenomenon that the

trout in its earliest post-larval stages is comparatively immune from the action of both Spurge-extract and tannic acid. Very strong solutions of both had to be employed before fatal effects were produced, and even then the time taken was much greater than for older specimens in weaker solutions. At later stages the larvæ gradually lost this power of resisting the action of the Spurge and tannin.

The time taken by the Spurge to produce fatal effects varies, of course, with the strength of solution employed. In the strongest possible extract death would occur in a few seconds. In a 20 per cent. solution of the fresh extract prepared as previously described death takes place within five minutes. As the solutions grow weaker the time taken becomes longer until with a solution of 0·01 per cent 4 hours elapse ere death occurs. The time varies, it should be said, with different specimens, but these represent the averages. The 0·01 per cent. solution is the weakest that need be considered for all practical purposes. This is 1 part in 10,000, and shows that a pool 100 feet long by 20 feet broad and 5 feet deep would prove fatal to all fishes within 4 hours if 1 cubic foot or 6 gallons of the fresh extract were poured into it. If, however, the plants are cut up, pounded, and placed directly into the pool, then a much smaller quantity would suffice. For, as has been previously stated, the "fresh extract" does not exhaust the possibilities of the plant, and several hours may elapse ere the poison is entirely dissolved out of it. The calculation is somewhat rough, but if we take an entire plant when fully developed to weigh about 400 grammes, then about a hundred plants are needed to poison a pool of the dimensions given. If the plants are pounded and placed in the pool, however, probably a fifth of this number would suffice.

If such a pool were almost stagnant, with little water passing through, it would be fatal to fishes, not merely for hours but days.

Reference to Table I will show that the Spurge-extract retains its deadly qualities for several days, and if the quantity thrown into the pool be not less than that quoted above, the fish there have little chance of escaping death. If the water be running in and out of the pool, the same effects will result from placing the plants at its upper end, for their presence there will keep the pool in a poisoned state for many hours and even a whole day. It is evident therefore that the salmon which lie in quiet pools waiting their opportunity to get up the rivers, are especially liable to be killed in this way. If they are swimming about in a broad reach of water they will be unaffected by any small current containing the Spurge-extract—unless it be in enormous quantity. But the peculiarity of the poison, causing as it does a loss of sensibility and consequent stupidity, affects the resting fish in such a way that it comes under the influence of the more fatal component of the Spurge before it is able to detect anything

wrong in the water. And once the gills are affected, there is little chance of the fish escaping. The whole habits of the salmon, in short, render it peculiarly liable to this form of poisoning.

Summary.

Chemical analysis of the Spurge-extract shows that it contains tannic acid. Experiments on the circulation in the lung and mesentery of the frog reveal a close similarity between the action of the Spurge-extract and of tannic acid. In the case of trout the similarity extends to the non-recovery of the fish in fresh water, after they have come under the influence of either Spurge-extract or tannic acid. The power of the Spurge-extract to produce fatal effects persists for several days without diminution. Twenty per cent. of the fresh extract is fatal within 5 minutes, whilst 0·01 per cent. takes 4 to 6 hours, and seems to be the smallest percentage which has fatal results. In the case of fishes, death is considered to ensue from the inflammation of the gills and consequent stasis of the circulation, set up by the action of the tannic-acid component of the Spurge-extract. The fresh extract is calculated roughly to contain about 1 per cent. of tannic acid, but on this estimation the Spurge-extract is fatal within a shorter period than the corresponding quantity of tannic acid. Hence, the percentage of tannic acid has been under-estimated, or some other substance or substances in the extract also aid in producing fatal effects.

"On Chemical Dynamics and Statics under the Action of Light." By Meyer Wilderman, Ph.D., B.Sc. (Oxon.). Communicated by Dr. Ludwig Mond, F.R.S. Received January 30,—Read February 13th, 1902.

(Abstract.)

Since the second half of the last century chemical statics and dynamics have developed into a veritable science of their own. The general law governing velocity of chemical reaction and chemical equilibrium in homogeneous systems is now known as the law of action of mass;* the law governing velocity of physical or molecular transformations in heterogeneous systems proves also to be of a general and simple nature: the velocity is directly proportional to the surface of contact of the reacting parts and to the remoteness of the system from the point of equilibrium;† the velocity of chemical

* Wilhelmy, Harcourt and Esson, Guldberg and Waage, Van't Hoff.

† M. Wilderman, 'Zeitschrift für Physikalische Chemie,' 1899, and especially 'Phil. Mag.,' July, 1901.

reaction in heterogeneous systems and chemical equilibrium in heterogeneous systems represent no phenomena *sui generis*, and the laws governing them are only combinations of the other two laws mentioned.* The laws concerning equilibrium have found their rational explanation and foundation in the researches of Horstmann, and foremost of W. Gibbs and Van't Hoff; the laws concerning velocity of reaction in homogeneous systems in the thermodynamic considerations of Van't Hoff. In all the above researches the phenomena of chemical reaction and of chemical equilibrium are the outcome of the intrinsic properties of matter, always existent in and inseparable from the same, and which we usually call chemical affinity or chemical potential.

But, as we know, a system can also be brought into a state of reaction, new systems, new equilibria, can be formed when energy from an external source, such as light or electricity, is introduced into the same. The effect of an electric current upon a chemical system is given by Faraday's law for electrolysis. The thermodynamic connection between chemical and electrical (and gravitation) energy is given by W. Gibbs.

What are now the laws governing the velocity of chemical reaction and chemical equilibrium when they are caused (not only influenced) by the introduction of light into the system? Is the velocity directly proportional to the amount of the light energy introduced or absorbed by the system in the unit of time, independent of the reacting masses or concentrations, *i.e.*, have we to expect here a law analogous to that of Faraday for electrolysis? or is the velocity of reaction some function of the reacting masses? What are the laws governing chemical equilibrium in light? It is evident that to furnish an answer to these problems careful experiments, quantitative measurements bearing straight upon the fundamental issues in question, were absolutely needed, and the help of the principles of thermodynamics in giving a rational explanation to the fundamental facts elicited by experiments had to be sought for.

The experimental work, started at the Davy-Faraday laboratory of the Royal Institution, in 1898, proved to be of very great difficulty, resolving itself into the solution of a series of other problems lying beyond the region of its immediate purpose, and only after two and a-half years of preliminary work the author succeeded for the first time in getting results, which allowed sure insight into the nature of the phenomena under consideration.

From the hundreds of reactions which are known to be caused or influenced by light,† there are not half a dozen which are suitable for quantitative measurements. The reaction used must be caused and

* See the same paper, 'Zeits. für Phys. Chemie,' 1899.

† Eder's 'Handbuch der Photographie.'

effected by light only, and not only be accelerated by the same; it must stop as soon as light is removed, *i.e.*, the phenomena under consideration must be studied in their purest form and not be complicated by other phenomena as well. Bunsen and Roscoe's reaction $H_2 + Cl_2 = 2HCl$ belongs to this class of reactions, but it cannot be used for a series of reasons, in the first instance because the chemical reaction is accompanied by no variation in volume. The reaction ultimately chosen by the author was that of Davy: $CO + Cl_2 = COCl_2$. This reaction is accompanied by a variation of the volume, 1 volume of $CO + 1$ volume Cl_2 giving 1 volume of $COCl_2$, *i.e.*, the velocity of reaction could be uninterruptedly studied from the variation of the manometer, connected with the glass bulb or quartz vessel containing the two gases. The CO and Cl_2 were used for the reaction dry. Under such circumstances the reaction goes on only with a small speed, so that to be able to investigate this reaction the problem of getting a powerful light (of 250 or 500 candle power), which would remain constant in intensity and composition for any length of time required, and which would be at the same time of great actinic properties, had to be solved. The light of the sun could not be used, and the arc light changes both in intensity and composition. Dr. Ludwig Mond suggested the use of the acetylene light for the purpose. Having first modified the "Icanto" acetylene generator of Thorn and Hoddle, so that it should not give greater variations in pressure than $1\frac{1}{2}$ per cent., instead of 10 per cent., and having further constructed a special burner, which gives a perfectly pure, smokeless, circular flame of 250 (or 500 candle power or more) on the space of about 1 inch, the author reduced the variations in the intensity and composition of the flame to about $1—1\frac{1}{2}$ per cent., by the use of a balance governor, and by taking special precautions to expel the air from the generator. By means of a thermopyle, galvanometer, regulating tap, &c., the intensity of the light could be easily adjusted to about 0·1 per cent. The author used a Rubens' thermopyle, having taken special precautions to make its indications concordant, and the thermoelectromotive force of the same in the dark not to exceed 1 or 2 mm., even when the room was heated by the powerful light of the acetylene burner for many hours. The galvanometer used was a sensitive Crompton of the D'Arsonval type. The adjustment of the flame was arranged to 0·1 per cent., though it could be arranged with a very much greater accuracy by the use of a more sensitive galvanometer (say, of a Nalder and Bros), or of a thermopyle of 1 square decimetre surface instead of 2 square centimetres.

The value of the read intensity of light was further checked by reading the deflections of the Crompton by means of a Clark cell and standard manganin resistances.

As it was found that exceedingly small traces of air or of water

vapour, which are most difficult to detect with ordinary chemical or physical means, are quite enough to diminish or accelerate the velocity of combination of Cl_2 and CO, quite extraordinary precautions in the preparation of perfectly pure CO and Cl_2 had to be taken.

The carbon monoxide was prepared in the manner given by Lord Rayleigh, *i.e.*, from sodium formate (35 gr.), two parts sulphuric acid (200 gr.), and one part of water (100 gr.). The other precautions taken were :—All the vessels were first heated and evacuated by means of a Töpler pump to about 0·01 mm. ; all the liquids used were first boiled in a vacuum so as to expel the air, carbonic acid, &c., from the same. To free the generated CO from the last traces of sulphurous acid and carbonic acid the gas was passed through a solution of caustic potash free from air, then through a U-tube containing solid caustic potash, and a long tube containing phosphorus pentoxide. The first portion of the prepared CO was removed, all the vessels having been again heated and evacuated, and the vessels then filled with CO ; before each experiment on light the CO was first removed from all the vessels and new CO prepared. Only quite freshly prepared CO could be used for the experiments.

The chlorine was prepared by heating of perfectly pure and dry cupric chloride ($CuCl_2$) free from hydrochloric acid, which, if it is once formed, is almost impossible to remove from the same. The $CuCl_2$ was prepared from precipitated copper and dry chlorine. The copper was first reduced in a tube with drawn-out ends in a current of hydrogen, the hydrogen removed by evacuation. A dry current of Cl_2, washed and well dried, was passed over the copper first in the cold, then while the copper was heated to about 250°, and then the tube sealed up at the two drawn-out ends. The tube was afterwards connected with another tube containing phosphorus pentoxide.

The vessels, in which CO and Cl_2 were prepared, were connected with the quartz vessel or the glass bulb in which the gases were exposed to light by means of capillary tubes, and the quartz vessel or glass bulb was connected with a manometer, the mercury of which was protected from the action of Cl_2 by a column of concentrated sulphuric acid free from air on the top in the capillary. The quartz vessel consisted of a short glass cylinder with glass rims, on which two quartz windows were fixed, and it contained a thermocouple to indicate the temperature of the gas during the reaction. Before the gases were introduced into the quartz vessel, the quartz vessel, manometer, &c., were evacuated and heated and washed with CO, the Cl_2 and CO completely removed (the chlorine by means of a removable pump), all the vessels again heated and evacuated to about 0·01 mm., the Cl_2 and CO again freshly prepared in the dark (on account of the properties of the deduction periods), introduced into the quartz vessel under special conditions, and here sealed up with a hand blow-pipe.

The quartz vessel with the manometer was then placed in a large water bath of a regulated and constant temperature, behind a quartz window, and here exposed to the acetylene light. At suitable intervals the temperature of the bath (read to 0°·01), the height of the mercury and sulphuric acid column in the manometer (read with the cathetometer), the barometer pressure and temperature (read with the vernier), and the intensity of light (*i.e.*, the deflections of the galvanometer by the Rubens' thermopyle and the thermoelectromotive force of the same) were observed. This allowed of knowing the exact amount of $COCl_2$ formed, or of CO and Cl_2 present in the gas mixture at any time τ.

In this manner the author succeeded in getting reliable and sufficiently regular curves (see diagram), which admitted an investigation of the phenomena under consideration. If we take the time τ (minutes) as abscissæ and the x or the amount of $COCl_2$ formed up to the time τ expressed in mm. pressure as ordinates, we get the curves of Tables II, III, IV, V, and the variation of x with τ, or $dx/d\tau$, gives the velocity of formation of $COCl_2$ from CO and Cl_2. Each of the above curves represents one and the same system. Curve (1), curves (2) and (3), (4) and (5) of Table II give the results obtained for the same system in three successive days, as the reaction of combination of Cl_2 and CO was further proceeded with. So also (1) and (2) of the rest of the tables belong for each table to two successive days.

The investigation of the above curves showed that the integral equation

$$\frac{1}{A-B}[\log(A-x_1)-\log(A-x_2)+\log(B-x_2)-\log(B-x_1)]:(\tau_2-\tau_1) = K$$

well expresses the same after the periods of induction had passed. This period of induction had passed, *e.g.*, in curve (2), Table II, at observation (9), in curve (4), Table II, at observation (51), in curve (1), Table II, at (4), in curve (2), Table III, at (10), &c.

In the above integral equation A and B are the quantities of Cl_2 and CO taken at the beginning, before the reaction was started (*e.g.*, A = 502·4 mm. partial pressure, B = 109·7 mm. partial pressure in the gas mixture of Table II, and A = 224·7 mm., B = 343·6 mm. in Table III, &c.); $A-x$ and $B-x$ are the quantities of Cl_2 and CO present in the system at the time τ, K is a constant.

The differential equation giving the law for the velocity of chemical reaction in light is thus $dx/d\tau = K(A-x)(B-x)$, i.e., *the velocity of combination of* Cl_2 *and* CO *in light, or the velocity of formation of* $COCl_2$, *is at the time* τ *directly proportional to the product of the reacting masses at the time* τ. *Since the chemical equation for the reaction is* $1Cl_2 + 1\,CO = 1\,COCl_2$, *this equation has in light the exact form which it ought to have*

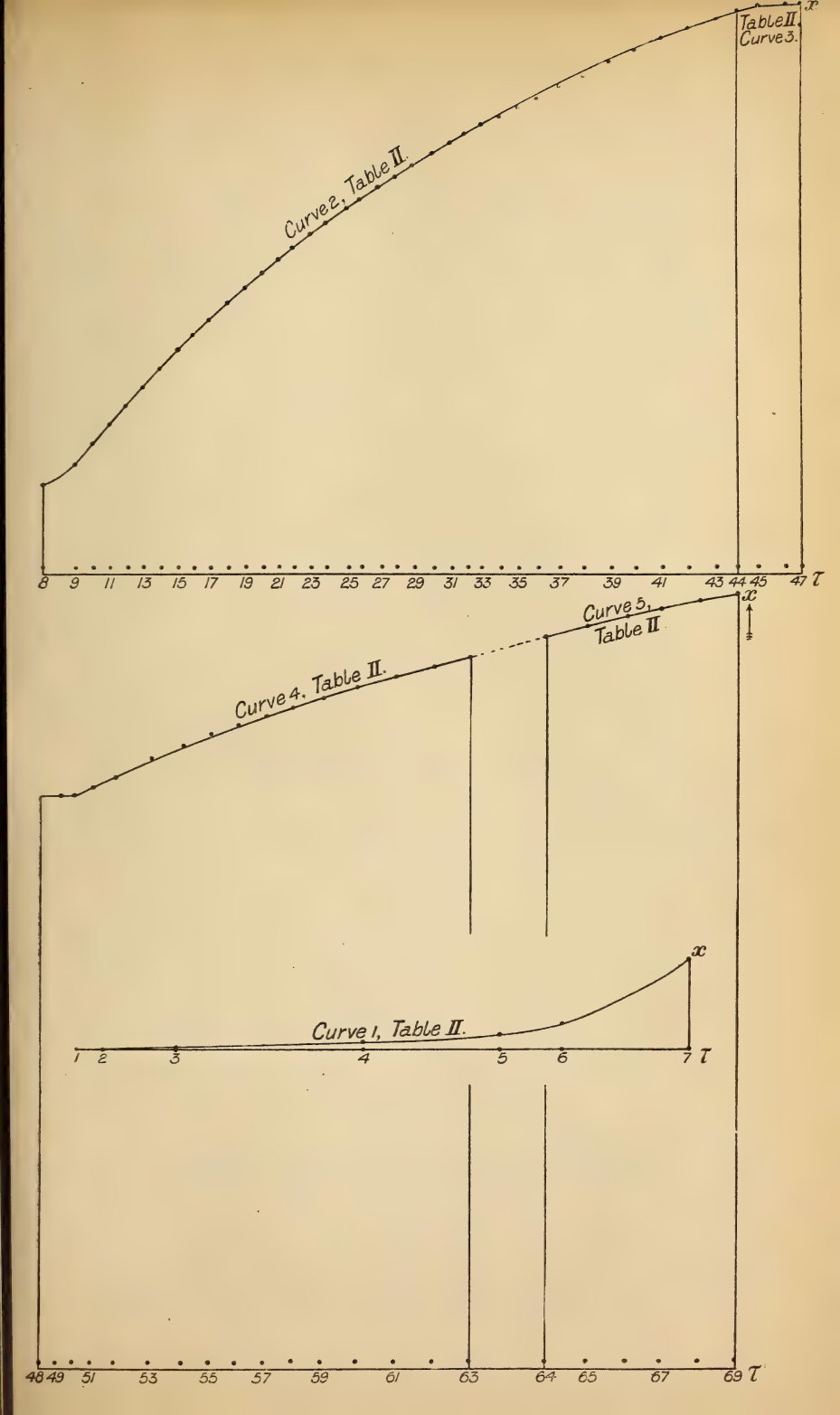
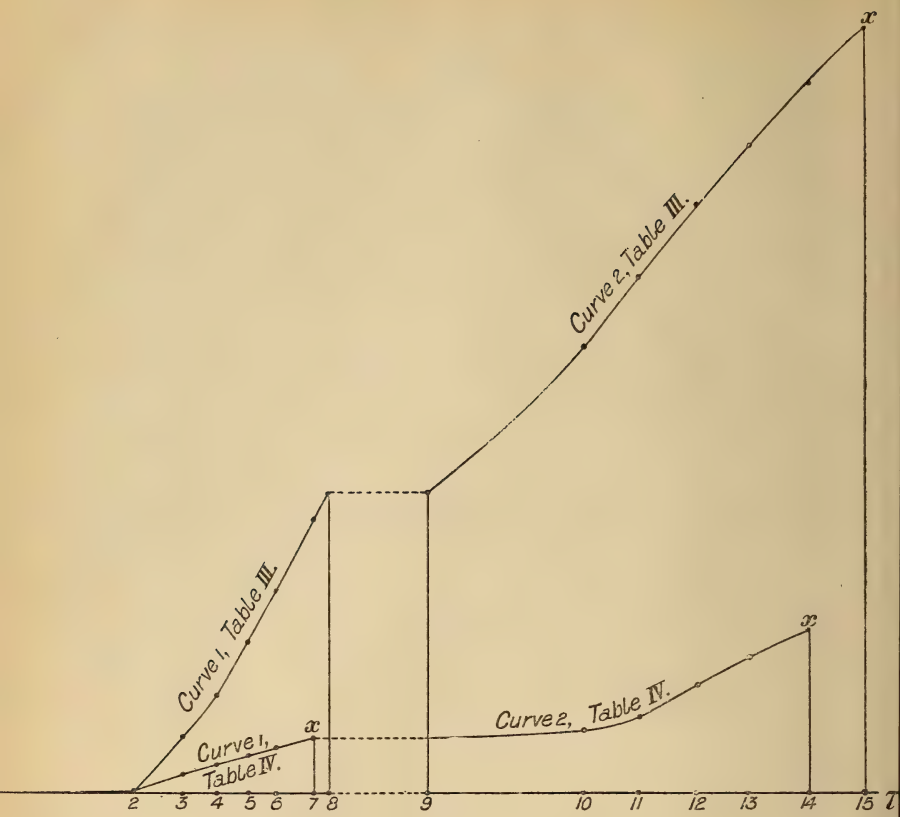
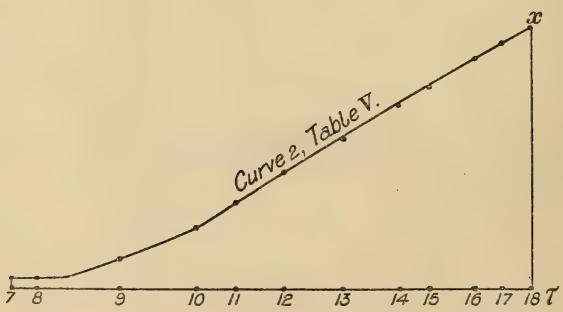
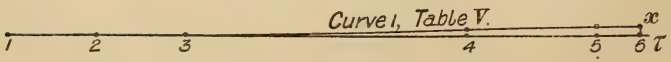


Table II. Curve 3.
Curve 2, Table II.
x
8 9 11 13 15 17 19 21 23 25 27 29 31 33 35 37 39 41 43 44 45 47 τ
Curve 5, Table II.
Curve 4, Table II.
x
x
Curve 1, Table II.
1 2 3 4 5 6 7 τ
48 49 51 53 55 57 59 61 63 64 65 67 69 τ


x
Curve 2, Table III.
Curve 1, Table III.
x
Curve 2, Table IV.
x
Curve 1,
Table IV.
2 3 4 5 6 7 8 9 10 11 12 13 14 15 τ


x
Curve 2, Table V.
7 8 9 10 11 12 13 14 15 16 17 18 τ


Curve 1, Table V.
x
1 2 3 4 5 6 τ

according to the laws of action of mass, if the same chemical reaction would go on in the dark as the outcome of those intrinsic properties of matter only, which we call chemical affinity or chemical potential. In the above equations K is the velocity constant, which gives the velocity of combination of Cl_2 and CO, under given conditions of experiment, when $A - x$ for Cl_2 is 1, and $B - x$ for CO is 1. It should be remarked that K in the above equations is the integral velocity-constant for acetylene light, because the value of (K) is for each wave-length different. Since, however, each wave-length has an equation of the same form $\frac{dx}{d\tau} = (K)(A - x)(B - x)$, the equation for light consisting of more than one wave-length remains the same, K or (K) is besides a function of the intensity of light, of the temperature, of the chemical nature of the reacting substances, and of the surrounding medium.

The combination of CO and Cl_2 was allowed to go on so far as 80·66 per cent. of the total amount of possible combination in Table II and 39·16 per cent. in Table III. On interruption of the light for different periods and exposing the system again to the light, the induction periods changed according to circumstances, but always the same velocity constant was observed. This shows that the above equation must truly represent the fundamental law underlying velocity of reaction in light. *The results obtained further show that the velocity of combination af* CO *and* Cl_2 *cannot possibly be regulated by a law analogous to that of Faraday, because this velocity is neither directly proportional to the amount of light passing through the system nor to the light absorbed by the same in the unit of time,* the calculated values which ought to have given a constant, falling for Table II from 25·2 to 4·14, and from 40 to 18·6, and for Table III from 509 to 99, and from 187 to 136.

The author further gives instances of chemical equilibria in homogeneous systems in light, in which one of the two opposite reactions goes on only in light, and the other in light and in the dark, and shows that *since chemical equilibrium in a homogeneous system, as is established, represents a reversible system, in which the two opposite reactions become equal, and since the velocity of each reaction separately follows the law of action of mass, the evidence is given by the above results that chemical equilibrium in homogeneous systems must also and can only follow in light the laws of action of mass, as they follow in the dark.*

Besides the above results, the author deals on the basis of the experiments made with the phenomena of *chemical induction* (when the system is exposed to light; first observed by Bunsen and Roscoe) and of *chemical deduction* (when the light is removed from the system), as well as with *the induction and deduction periods of energy*; he also shows the enormous influence of small traces of air and of water vapour upon the velocity of the reaction (the first retards, the second accelerates the same).

The laws concerning chemical statics and dynamics in light are here elicited in an experimental way and are of a formal nature. In a paper now in course of preparation, "On the Connexion between the Light Energy (or the Energy of Electric Waves) absorbed by a System and its Chemical, Thermal, Mechanical, and other Energy," the author shows that the fundamental results, which he first elicited here in an experimental way, as well as some other experimental results bearing upon other fundamental issues in other regions of research, can, among other things, be deduced from thermodynamics, and are a necessary consequence of the same. In the same manner the induction and deduction periods follow as a necessity from the same general thermodynamic conceptions. This paper will be communicated in due course in the near future.

"On a Convenient Terminology for the Various Stages of the Malaria Parasite." By E. Ray Lankester, M.A., F.R.S., Director of the Natural History Department, British Museum. Received February 20,—Read March 6, 1902.

I have found it necessary in labelling a series of models of the malaria parasite in the Central Hall of the Natural History Museum to use as simple and clear a terminology as possible. I think that this terminology will be found useful by others who are perplexed by such terms as "sporozoites," "blasts," "ookinetes," "schizonts," "amphionts," and "sporonts"—terms which have their place in schemes dealing with the general morphology and life-history of the group Sporozoa, but are not, as experience shows, well suited for immediate use in describing and referring to the stages of the malaria parasite.

It is necessary to treat the malaria parasite from the point of view of malaria; that is to say, to consider its significant phases to be those which it passes in the human blood. In reality its mature condition and most important motile, as well as its most prolific reproductive, phases are passed in the body of the mosquito.

1. The malaria-germ which is brought by the stab of the Anopheles into the human blood-vessels is a reproductive particle, a *spore*. It is needle-like in shape, and might be named in reference to its form (*e.g.*, oxyspore or raphidiospore), but the most important fact about it for description and comparison is that it has been formed *outside* the human body, and is introduced as a strange element into the human blood by the agency of the mosquito. I therefore call it the Exotospore.

2. The Exotospores (probably as many at a time as forty or fifty) enter the blood by the agency of the mosquito's stab and immediately

penetrate, each one, a red corpuscle. The history of this process has not been observed. As soon as it has entered a red corpuscle the exotospore loses its needle-like shape and becomes amœbiform. I apply to it the name I proposed some years ago for similar amœbiform spores in other Protozoa, namely, AMŒBULA.*

3. The Amœbula exhibits amœboid movements within the red corpuscle, enlarges and finally breaks up into spherical spores, which are liberated with destruction of the red corpuscle. It seems to me unnecessary to have a special name for the star-like or other condition of the Amœbula when in course of breaking up into spores; but the spores so produced require a special name which shall emphatically distinguish them from the Exotospores. I call them the ENHÆMOSPORES, in reference to the fact that they are produced by a process of division which occurs *in* the blood of the malaria-stricken human being.

4. The Enhæmospores penetrate fresh red blood-corpuscles, and after a certain growth as amœbulæ break up into a new crop of Enhæmospores, by which the infection of the red corpuscles is extended. This process appears to go on for several generations and for a varying duration of time. But owing to conditions and at a period of the infection which has not been precisely ascertained, some (or all ?) of the amœbulæ derived from Enhæmospores cease to break up into spores. Instead of carrying out that process they enlarge, and in the case of the æstivo-autumnal parasite (*Laverania præcox*) become sausage-shaped or, as it has been termed, crescent-shaped. This change of form is accompanied by a destruction of the red corpuscle and the formation of granules of dark pigment within the parasite. It seems best to term this phase the "CRESCENT" or "CRESCENT-SPHERE," the latter term being applicable to those species in which the form is not markedly crescentic.

5. The crescents or crescent-spheres remain quiescent in the human blood. They are, however, of two different natures—male and female. It is not possible to distinguish with any certainty the male from the female crescents whilst they remain in the human blood-vessels. But it is these bodies which are destined to be swallowed by the Anopheles mosquito, and to carry on further the life-history of the parasite.

The crescents are therefore the sexual phase of the parasite. When the crescents are swallowed by a mosquito (of an appropriate species), they undergo two different modes of development, determined by the fact of their sex. Both sexes become spherical, and may now be called respectively "EGG-CELL," and "SPERM-MOTHER-CELL."

From the periphery of the SPERM-MOTHER-CELL, now floating in the mosquito's stomach, there are developed with surprising rapidity six or seven SPERMATOZOA, which for a time remain attached to the residual

* 'Encycl. Britann.,' Article "Protozoa."

mass (or SPERM-BLASTOPHOR) of the sperm-mother-cell. Complete cytological study of this development is still wanting, but it appears that the spermatozoa are true spermatozoa, like those of the higher animals, and have the same relation to the mother-cell from which they develop as is the case in such an animal as the Earth-worm.

The EGG-CELL, now also floating in the mosquito's stomach, apparently gives rise to one, and possibly to two, polar bodies, but the observations on this point are, as yet, insufficient.

Fertilisation of the egg-cell now takes place in the gnat's stomach. A single spermatozoon penetrates and fuses with each egg-cell.

The fertilised egg-cell is spoken of as a "zygote"; it is also described as the sexually produced embryo.

6. The ZYGOTE or SEXUALLY PRODUCED EMBRYO remains unicellular, but increases in size and becomes pyriform. It exhibits active movements of expansion and contraction in the line of its long axis, and also a quick movement of its narrower end alternately to either side. This is the largest growth of the individual cell attained to in the series presented by the life-history of the malaria parasite. It has been called "vermiform" and "vermicule" (Ross), and I adopt this name for it, viz., the VERMICULE. The vermicule is the dominant individual form in the history of the malaria parasite, endowed with greater size, power, and activity than other phases. It corresponds, not only in this respect, but also in its position in the life cycle, to the large often active cells of the Gregarinidea, which I proposed some time ago to call the Euglena-phase.*

It is worthy of note that in the size and activity of the Vermicule, the Hæmaosporidia—the order of Sporozoa which embraces the malaria parasite—come nearer to the Gregarinidea than they do to the Coccidiidea, though in the existence of a sexual generation absent in Gregarinidea, they agree with the Coccidiidea.†

The vermicule now pushes its way through the tissues of the gnat's stomach and in the blood sinuses outside the stomach becomes spherical. It enlarges and nourishes itself on the insect's blood, and forms a spherical CYST, or structureless transparent envelope. This cyst is destined to enlarge, with vast increase of its living contents.

The living cell within the cyst breaks up by a definite process to form eventually an immense number of exotospores, the stage with which the present description commenced. The CYST would most conveniently be called a "sporocyst," since, as so often happens in Protozoa, it is formed purely and simply in relation to the quiescence

* 'Encycl. Britann.,' Article "Protozoa."

† A sexual phase has been described in the Gregarine Stylorhynchus by Léger since this paper was written. It occurs at an unexpected point in the cycle: two encysted full grown "Sporonts" are stated to produce the one egg-cells the other spermatozoids!

of the organism and its division into numerous reproductive spores. Unfortunately, the word "sporocyst" has been employed recently by writers on the Sporozoa for the small capsules containing one or two to eight elongated spores which used to be called "pseudonaviculæ," and are formed *within* such larger cysts as that now in question. The word "cyst" should have been reserved for the larger more general protective envelope, and the "pseudonaviculæ" might have been called "sporo-thekæs." In any case, I think we may call the cysts in which the vermicules of the malaria parasite enclose themselves "SPORE-CYSTS" or "SPORE-FORMING CYSTS." The name "oocyst," applied to them by some writers, is simply misleading.

7. The spore-cysts lying outside the stomach wall of the mosquito bathed in the insect's blood receive abundant nourishment. The single-celled vermicule enclosed undergoes rapid changes; it increases greatly in volume and breaks up by normal cell division (? the earliest steps have yet to be studied) into a number of SPORE-MOTHER-CELLS. In the process of this division and the later stages of the final development of the "spores" (exotospores), the "spore-forming cyst" increases in size to twenty times its initial diameter.

The spore-mother-cells are set closely together in the cyst; they are of polygonal shape, owing to pressure, and each has its nucleus. Finally they give rise, each spore-mother-cell, to a crop of filiform spores (exotospores) which have the same relation to the spore-mother-cell as spermatozoa have to a sperm-mother-cell, viz., they form on the outside of the spore-mother-cell as outstanding processes, carrying away all the chromatin of the mother-cell and leaving in the centre or to one side a "residuary body," a "spore blastophore" similar to the "sperm-blastophore" of spermatozoon-development.

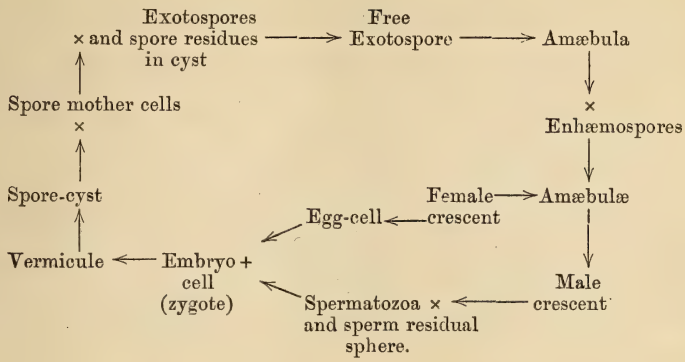
Thus we are brought back to the needle-like exotospores with which we started.

The spore-holding cysts burst and liberate the exotospores into the blood of the mosquito. Thence they readily pass into the ducts of the salivary gland, and so are conveyed by the mosquito's stabbing beak into human beings. A point in this connection is the definite ejection by the mosquito of the secretion of its salivary gland into the punctured wound which it makes in the human skin. There can be no doubt that such an ejection takes place. The leech ejects a secretion on to the wound caused by its bite which has the property of preventing the coagulation of the blood. It is possible that the mosquito and other blood-sucking flies may use the salivary secretion for the same purpose. It is obvious that unless there were some injection into the wound on the part of the fly, the chances of infection of the bitten animal by the parasites carried by mosquitoes or tzetze fly would be very small.

Our cycle of forms with the names here made use of may be

	Malaria.	Coccidium.	Gregarina.
1.	Exotospore, free in human blood (" Blast " of some authors.)	Sporozoite	Sporozoite. (Filiform young.)
2.	Amœbula, in red corpuscles	Schizont	Amœbula.
3.	Enhæmospore, ditto, and in blood	Merozoites, formed by schizogony	Schizogony rare: sexual stages NOT OBSERVED and probably WANTING.
4.	Crescent, in human blood	Gametocytes	
	a. Male	Microgametocyte	
	b. Female	Macrogamete	
5.	Sperm-mother-cell, in gnat's stomach	Microgametocyte	
6.	Egg-cell, in gnat's stomach	Macrogamete	
7.	Spermatozoon, in gnat's stomach	Microgamete	
8.	Zygote or embryo-cell, in gnat's stomach	Young oocyst (sporont)	
9.	Vermicule, in gnat's stomach	WANTING (Called " ookinete " or " kineto-sporont " in the nomenclature of this column.)	Full-grown motile " gregarine." (Euglenoid phase.)
10.	Spore-cyst, in blood-sinus outside gnat's stomach	Older (but not larger) oocyst or sporont	Cyst enclosing one or two full-grown sporonts.
11.	Spore-mother-cells in cyst, in blood-sinus outside gnat's stomach	Sporoblasts (sporogony)	Sporoblasts. (? Conjugation in *Lankesteria Ascidiæ*. Spermatozoa and ova in Stylorhynchus.)
12	Exotospores in cyst, in blood-sinus outside gnat's stomach	Sporozoites enclosed in small groups in sporocysts within the bigger oocyst.	Sporozoites enclosed in capsules, called " pseudonaviculæ " or " sporocysts."
21.	Free exotospores, in gnat's salivary duct	Free sporozoite	Free sporozoite.

written as below. The sign × is used to indicate fissile multiplication, and + to indicate fusion, while → merely indicates continuity.



I also give a list of the names here used with reference to the occurrence of the forms indicated in man or in gnat and an indication of the corresponding stages in a Gregarina and a Coccidium. In the column belonging to coccidium I have employed the generalised physiological nomenclature accepted by special students of the Sporozoa (Schaudin, Lühe, &c.)

THE CROONIAN LECTURE.—"On Certain Chemical and Physical Properties of Hæmoglobin." By ARTHUR GAMGEE, M.D., F.R.S., Emeritus Professor of Physiology in the Owens College. Lecture delivered March 13, 1902,

(Abstract.)

This lecture consists of two parts, of which the first is bibliographical and critical, the second experimental.

PART I.—*Bibliographical and Critical.*

The author commences by stating that a peculiar interest—the parallel of that which in the plant organism belongs to chlorophyll—attaches to hæmoglobin, for, unlike any other chemical component of the animal body, in virtue of its special chemical and physical attributes, this remarkable substance may in the strictest sense be said to possess a definite and unique physiological function.

The author then discusses certain facts in reference to hæmoglobin and its products of decomposition which have a close bearing on his researches, or which possess special interest in the light of work which

has been recently done. He does so under the following heads :—(1.) The question of the identity of hæmoglobin throughout the animal kingdom, (2.) The nature of the albuminous body which is linked to the coloured iron-containing group in hæmoglobin—*Globin* (*Hæmato-Histon*). Under this heading he discusses the general properties of the histons and their relation to the simplest of all the albuminous bodies, the *Protamines*. (3.) The products of the decomposition of the iron-containing molecular group in hæmoglobin. Under this heading the author directs special attention to the researches of Schunck and Marchlewski, which have shown by the oxidation of phyllotaonin, one of the decomposition products of chlorophyll, a substance, PHYLLO-PORPHYRIN, is obtained which is probably identical with hæmatoporphyrin. Under this, as also under the preceding head, the author discusses various questions in reference to the seat of the synthesis of hæmoglobin.

PART II.—*Experimental.*

(1.) *Extension of Previous Observations on the Absorption of the Ultra-violet Rays of the Solar Spectrum by Hæmoglobin.*

The author refers briefly (illustrating his remarks by demonstrations) to his researches, previously published, into the absorption of the extreme violet and ultra-violet rays of the solar spectrum by hæmoglobin, its compounds, and certain of its derivatives. The region of the solar spectrum which he formerly investigated was that comprised between the lines F and Q (4861—3280).

He now examines the question whether oxy-hæmoglobin presents definite absorption for light of shorter wave-lengths. Soret, whose observations were not conducted with solutions of hæmoglobin but merely with diluted blood, observing by the aid of his fluorescent eye-piece the cadmium spark spectrum, found that diluted blood, in addition to the absorption band in the extreme violet, exhibited two additional bands. One of these, coinciding with the 12th cadmium line (3247), he considered to be probably due to hæmoglobin; the other, coinciding with the 17th cadmium line (2743), he assumed to be caused by serum albumin, his observations having previously shown that all albuminous and albuminoid bodies, with the exception of gelatin, are characterised by an absorption band in the position of the 17th cadmium line.

Employing solutions of many times crystallised oxy-hæmoglobin of great purity and of varying concentration, and with the aid of the sparks of a powerful induction coil, the author has obtained a series of photographs of the cadmium spark spectrum with and without the interposition of the solutions. The examination of these photographs shows that solutions of oxy-hæmoglobin which are sufficiently trans-

parent to allow the ultra-violet spectrum of cadmium to be photographed, present no absorption bands corresponding either to the 14th or the 17th cadmium lines. The absorption band observed by Soret in correspondence with line 14 is, therefore, not due to the blood colouring matter, but to some other organic constituent present in the blood.

(2.) *Behaviour of Oxy-hæmoglobin and CO-hæmoglobin in the Magnetic Field. The Intense Ferro-magnetic Properties of Hæmatin and Hæmin.*

Having referred to his researches communicated to the Royal Society, in June, 1901, and illustrated the main facts by actual demonstrations, the author discusses (1) observations on the influence of temperature on the behaviour of oxy-hæmoglobin in the magnetic field: (2) observations on the ferro-magnetism of the ferro-albuminates.

(3.) *The Specific Conductivity of Solutions of Oxy-hæmoglobin.*

The author next examines the question of the specific conductivity of solutions of pure oxy-hæmoglobin.

In this research he has worked exclusively with the hæmoglobin of the horse, and, following substantially the method of preparation which in Zinoffsky's well-known investigation yielded the purest product, he succeeded in obtaining crystallised oxy-hæmoglobin of remarkable purity, the ash of which consisted solely of oxide of iron with an indeterminable trace of P_2O_5 and contained no trace of chlorine. He employed oxy-hæmoglobin three times recrystallised, and in addition to the thorough washing by decantation with 20 per cent. alcohol after each successive crystallisation, he treated the ultimate product many successive times with large quantities of pure distilled water having a conductivity which never exceeded $10^{-6} \times 2{\cdot}5$, the washed hæmoglobin being separated by the aid of the centrifuge. The solutions which formed the subject of investigation were made by dissolving the mass of moist hæmoglobin crystals in pure distilled water of 35° C., and cooling the solution thus obtained as rapidly as possible.

In his determination of specific conductivities, the author employed the method of Kohlrausch. The bridge was Kohlrausch's metre bridge, of which the platinum-iridium wire was 3 m. in length. This bridge is furnished with resistances of 1, 10, 100, and 1000 ohms, the precision of which had been kindly determined some years ago for the author by Dr. Glazebrook, F.R.S. The resistance vessels employed were those known after the name of Arrhenius, and the temperature was kept constant by immersing them in one of Ostwald's thermostats furnished with a windmill stirrer.

After a laborious investigation on this branch of the subject, the author has arrived at the following conclusions:—

(1.) Although solutions of oxy-hæmoglobin possess a low conductivity, this is very much higher than has been found in the previous observations of Stewart, all of which were made at 5° C.

(2.) The conductivity of solutions of oxy-hæmoglobin increases rapidly with increase of temperature, and undergoes remarkable and permanent changes when the solution is kept for even short periods at any temperature above 0° C.

These results explain the impossibility of obtaining data which can be considered reliable concerning the *absolute specific resistance* of solutions of oxy-hæmoglobin.

The following numbers expressed in reciprocal ohms represent the mean of the author's results on the specific conductivity of solutions of oxy-hæmoglobin :—

	1. Contains 3·07 per cent. of O_2Hb (or 1 gramme molecule in 542900 grammes).	2. Contains 2·235 per cent. of O_2Hb (or 1 gramme molecule in 745800 grammes).
T.	Conductivity.	Conductivity.
0°	$10^{-5} \times 2{\cdot}626$	$10^{-5} \times 2{\cdot}23$
18°	$10^{-5} \times 4{\cdot}432$	$10^{-5} \times 3{\cdot}25$
25°	$10^{-5} \times 5{\cdot}19$	$10^{-5} \times 4{\cdot}27$
39°	.. ..	$10^{-5} \times 7{\cdot}47$

4. *The Results of the Electrolysis of Oxy-hæmoglobin.*

1. Continuing the researches contained in his first communication to the Royal Society on this subject, the author finds that when pure solutions of oxy-hæmoglobin are subjected to electrolysis, there occurs a separation of oxy-hæmoglobin in a colloidal, but perfectly soluble form. He has worked with currents of from 12 to 24 volts, and the intensity of the electrolysing current measured by a milliampere-meter placed in the circuit has varied in different experiments between 0·1 and 3·0 milliamperes.

2. By employing an electrolytic cell in which the anode is separated from the kathode by an animal membrane (sheep's intestine or pig's bladder), it is seen that the first action of the current is to cause a separation of colloidal hæmoglobin in the anode cell. This colloidal hæmoglobin falls as a beautiful red cloud, leaving a perfectly colourless, supernatant liquid. On stirring it instantly dissolves.

3 The further action of the current is to cause a rapid and entire transfer of the colloidal hæmoglobin from the anode to the kathode cell. With an electrolytic cell, of which each compartment had a

width of 5 mm. and contained 2·5 c.c. of a 1 per cent. solution of O_2Hb, complete precipitation and transfer occurs within 60 minutes.

4. On reversing the direction of the current by means of a commutator, the hæmoglobin returns again in the direction of the positive current into the original cell from which it started.

5. The author adduces evidence which proves that the precipitated colloidal, but yet perfectly soluble, hæmoglobin represents the undecomposed molecule of the blood colouring matter.

6. The probable nature of the process which occurs under the influence of the current is discussed, as well as the character of the process which leads to the transfer of the hæmoglobin in the direction of the positive current. This process the author considers to be of the same nature as the phenomena studied by Quincke under the name of electro-endosmose.

7. The author directs special attention to the importance of the facts which he has elicited in reference to the colloidal yet soluble form of oxy-hæmoglobin. He points out that all which has been said with regard to oxy-hæmoglobin applies to CO-hæmoglobin.

A typical colloid in the sense of its absolute indiffusibility through animal membranes and parchment paper, oxy-hæmoglobin differs, however, from most colloids in the facility with which it crystallises. Hitherto we have known it in its crystalline condition and in solution in water. Now in its third or colloidal form the analogy with such a colloid as silicic acid is rendered complete.

The discovery of this form of hæmoglobin enables us to form a conception of the state in which the blood colouring matter is probably contained in the blood corpuscles. We have known that the amount of hæmoglobin contained in the corpuscles is so large that in most animals at least the whole of the water of the blood would not be sufficient to dissolve it. It was perfectly obvious, therefore, that it did not exist in the corpuscles in a state of solution, and the opinion has generally been held that these contained some unknown compound of oxy-hæmoglobin with a constituent of the stroma. It seems highly probable that in the red blood corpuscle hæmoglobin may be merely present in its colloidal form.

Finally the auther points out that the remarkable facility with which the new colloidal form of hæmoglobin traverses such permeable membranes as the animal membranes and even parchment paper, when its solutions are subjected to electrolysis, suggests to physiologists the possibility that certain of the phenomena of absorption in the animal body may be closely connected with electromotive changes in the tissues concerned.

"On the Development of the Layers of the Retina in the Chick after the Formation of the Optic Cup." By JOHN CAMERON, M.B., Ch.B. (Edin.). Communicated by Professor MCINTOSH, F.R.S. Received February 6,—Read March 20, 1902.

(Abstract.)

The inner wall of the retinal cup in a 4th-day chick has exactly the same structure as the wall of the embryonic cerebral vesicles or spinal cord at the same stage of development. Thus all the structures which His has described in the wall of the embryonic spinal cord can be also recognised in the inner wall of the retinal cup, and may therefore receive similar names. (1.) A network (the myelospongium), which is produced by the union of the processes of cells called spongioblasts. The outer and inner extremities of the myelospongium network fuse to form the external and internal limiting membranes respectively (the external limiting membrane of the retina corresponds to the internal limiting membrane of the embryonic spinal cord or cerebral vesicle as it is next to the cavity of the original optic vesicle).

(2.) In the meshes of the myelospongium are two kinds of cells—1st, germinal cells, which are found only under the external limiting membrane; 2nd, neuroblasts, which are formed from division of the germinal cells and give rise to the ganglion cells and the cells of the inner and outer nuclear layers (the cells of the outer nuclear layer are the youngest cells of the retina).

Up to the 8th day, the inner wall of the retinal cup grows greatly both in thickness and surface area. On the 8th day of incubation the internal molecular layer appears, and on the 9th day the external molecular layer. These two layers first show themselves in the central point of the retinal cup, and extend forwards in all directions towards the anterior margin of the cup. The outer and inner molecular layers do not extend into the anterior one-fourth of the retinal cup, and this portion shows the simple arrangement seen before the 8th day; but in the posterior three-quarters, the external and internal nuclear layers and the ganglion cell layer are mapped out by the two molecular layers.

During the 8th day a rearrangement of the myelospongium occurs, and consists in the lateral offshoots of the fibres being absorbed for the most part in the region of the future internal nuclear layer; but in the region of the internal molecular layer the network becomes finer and much denser, and thus pushes the cells away on either side of it. The internal molecular layer appears before any processes from the ganglion cells or the internal nuclear layer have grown into it, and

therefore its first appearance is not due to the growth of the processes from these cells into it. The three streaks which develop in the internal molecular layer are due to a denser texture of the myelospongium at these places, on either side of which the fine arborisations from the cells in the ganglion layer and internal nuclear layer tend to accumulate.

The first appearance of the external molecular layer is explained as follows:—The myelospongium fibres bifurcate near their outer ends and neighbouring branches fuse with one another, so that the external molecular layer has at first an irregular outline. Later on, when the retinal wall becomes thinner, then this layer has a straighter outline. The persisting radial fibres of the myelospongium become the fibres of Müller.

In the internal nuclear layer, three kinds of cells can be distinguished at the 12th day of incubation: 1st, a single row of cells with clear nuclei, which lie next to the external molecular layer (basal cells); 2nd, several layers of small bipolar cells with external and internal processes passing into the external and internal molecular layers respectively; 3rd, a set of cells, usually arranged in three rows and giving off processes into the internal molecular layer (amacrine cells). From the 10th to the 12th day the ganglion cells spread out to form one layer, and the bipolar cells of the internal nuclear layer also spread out so that the retina becomes thinner. At the 10th day two kinds of cells can be recognised in the external nuclear layer—1st, cells which extend from the external limiting membrane to the external molecular layer, and are of the same breadth throughout (the rod cells); 2nd, cells which taper towards the external molecular layer (the cone cells). The rods and cones develop on the 12th day as outgrowths from these cells. The rods, however, appear a little earlier than the cones, and form globular projections which soon become flask-shaped. One cone-element appears usually between two flask-shaped rod-elements, and are therefore forced to assume a cone shape, but from them a fine filament is forced between the rod-elements and forms a minute spherical swelling immediately beyond. In most of the rod and cone cells a spherical body can be seen lying between the nucleus and the external limiting membrane on the 14th day. The rods and cones increase in length, due to protrusion of more and more protoplasm from the rod and cone cells, and this spherical body (the rod and cone ellipsoid) is borne along with the later protrusions of protoplasm and comes to occupy the inner segments of the rods and cones. Thus the outer segments of the rods and cones are the first to be protruded and lastly the inner segments. The oil globule develops in the protruded portion of the cone-element on the 15th day, and becomes coloured red or yellow on the 17th or 18th day. When first developed the rods about equal the cones in number, whereas

in the adult bird the cones are most numerous. Some of the bipolar cells from the internal nuclear layer emigrate through the external molecular layer, and develop into young cones. These investigations on the retina led to an inquiry as to what relationship there might be between the rod and cone cells, and the ciliated epithelium of the cerebral ventricles, and it is found that these are homologous structures. The germinal cells which line the cavities of the cerebral vesicles remain there after they cease to divide, and from them cilia develop. The inner ends of the myelospongium fibres do not develop cilia as is usually described.

The processes of the hexagonal pigment cells appear on the same date and at the same spot as the rods and cones and do not at first contain pigment granules. They resemble pseudopodia in their mode of development, and also, after full development, they behave like pseudopodia, for they elongate and retract under the influence of light and carry the pigment granules with them (the granules having no inherent power of movement).

The inner wall of the retinal cup has been seen to have the same structure as the wall of the cerebral vesicle in the early stages, and even later (as at the 8th day) some resemblance can still be drawn between them.

"The Classification of the Elements." By Henry E. Armstrong, V.P.R.S. Received March 5,—Read March 20, 1902.

Although no direct evidence acceptable to chemists has been adduced which in any way justifies the belief that the elements are decomposible, it is impossible to resist the conclusion that they are genetically related —so closely in many respects do they resemble a series of related compounds, especially when regarded from the point of view of the organic chemist. The generalisation known as the *Periodic Law* is in itself a justification of this view: the manner in which interrelationship becomes manifest when they are classified in accordance with its canons, being probably the strongest of all the arguments which can be cited as tending to show that the elements are compounds—but compounds very different from those with which we are accustomed to deal. Even in the form in which it was put forward by Mendeleeff, however, the periodic generalisation is but a first approximation: and the great Russian has himself pointed out that it needs improvement and development.* As chemists are beginning to recognise this,† I venture to submit a scheme of classification which I have been led to draw up in

* Faraday Lecture, 'Trans. Chem. Soc.,' 1889, p. 656.

† Comp. Biltz, 'Deut. chem. Ges. Ber.,' 1902, p. 562.

writing an article for the forthcoming Supplement to the 'Encyclopædia Britannica.' The article, I may say, was sent to press in May, 1900, and the first proof before me is dated November 20, 1900.

The suggestion of an octave of elements having been made in early days, the tendency to adopt an octavo system has been irresistible. It is difficult now to discover any real justification of such a practice. As there is frequently a difference of (about) a single unit between the atomic weights of contiguous elements, and as the "homologous" elements among those of lower atomic weight differ by (about) sixteen units—I assume that the "elementary difference" may be (about) a single unit, and that the first "horizontal period" comes to an end with oxygen: in other words, that there are sixteen "vertical series" of homologous elements.

In the table on pp. 88—89, the elements are entered under whole numbers without regard to their exact atomic weights. The dominant principle on which the arrangement is based is that of maintaining elements which belong to the same family in the appropriate columns. The chemist cannot adopt any other mode of arrangement in view of the uncertainty which attaches to many of the atomic weights.

I have not hesitated to assume that the molecules of argon and similar elements are polyatomic like those of nitrogen—their nearest ally in outward behaviour—and have regarded them as *diatomic*, as the molecules of all the ordinarily gaseous elements are of this order of complexity. It has always been my opinion that they are elements of intense activity, the atoms of which so fully satisfy each other that no residual affinity is manifest in the molecules. From this point of view the appearance of argon immediately after fluorine is quite in order.

Turning to the several periods, the first calls for little remark. Helium is put next to hydrogen—and it may well be that elements somewhat like helium and argon will be discovered which will fall into positions 3, 4, and 18; in fact, this region may well turn out to be the nesting-place of such elements. Beryllium is shown separated from lithium and magnesium, but not to an extent which need give rise to objection. It is perhaps not beyond the region of possibility, however, even in the case of an element with so low an atomic weight, that isomorphous mixtures may have passed as pure substances: this possibility has not been sufficiently taken into account in preparing the material for determinations of atomic weight.

In the second period, although it does not come immediately below nitrogen, phosphorus is very near to that element—and sufficiently so to correspond with their very close relationship.

In the third period, when scandium is passed, it is impossible to proceed by units: to bring titanium into position it is necessary to step down five units. Proceeding from titanium, however, vanadium and chromium fall into appropriate positions.

1 **H**	2 **He**	3	4	5	6	7 **Li**	8
17	18	19 **F**	20 **A**	21	22	23 **Na**	24 **Mg**
33	34	35 **Cl**	36	37	38	39 **K**	40 **Ca**
53	54	55 **Mn**	56 **Fe** 57 58 59 **CoNi**	60	61	62	63 **Cu** 64 65 **Zn**
78	79	80 **Br**	81	82	83	84 85 **Rb**	86 87 **Sr**
98	99	100	101 102 **Ru** 103 **Rh** 104 105 **Pd**	106	107	108 **Ag**	109 110 111 112 **Cd**
125	126	127 **I**	128	129	130	131 132 133 **Cs**	134 135 136 137 **Ba**
						(**Sm** 150, **Eu** 151, **Gd** 156,	
			191 **Os** 192 193 **Ir** 194 195 **Pt**	196	197	198 **Au**	199 200 **Hg** 201

9 **Be**	10 **Ne**	11 **B**	12 **C**	13	14 **N**	15	16 **O**
25	26	27 **Al**	28 **Si**	29	30	31 **P**	32 **S**
41	42 **Kr**	43 **Sc**	44				
			45				
			46				
			47				
			48 **Ti**	49	50	51 **Vd**	52 **Cr**
66	67 **X**	68					
		69					
		70 **Ga**	71				
			72 **Ge**				
			73	74	75 **As**	76	77 **Se**
88	89	90 **Yt**	91 **Zr**				
			92				
			93	94	95	96 **Nb**	97 **Mo**
113	114	115 **In**	118 **Sn**				
		116	119				
		117 **Vc** (?)	120	121 **Sb**	122	123	124 **Te**
138 **La**							
139							
140 **Ce**							
141 **Pr**							
142							
143							
144 **Nd**							
Tb 163, **Er** 166, **Yb** 173.)						183 **Ta**	184 **W**
202	203	204 **Tl**	205				
			206				
			207 **Pb**	208 **Bi**			
			232 **Th**				240 **U**

Table II.

1 **H**	2 **He**	3	4	5	6	7 **Li**	8
17	18	19 **F**	20 **A**	21	22	23 **Na**	24 **Mg**
33	34	35 **Cl**	36	37	38	39 **K**	40 **Ca**
53	54	55 **Mn**	56 **Fe**				
			57				
			58				
			59 **CoNi**	60	61	62	63 **Cu**
							64
							65 **Zn**
							66
78	79	80 **Br**	81				
			82	83	84	85 **Rb**	86
							87 **Sr**
97	98	99	100				
			101				
			102 **Ru**				
			103 **Rh**				
			104				
			105 **Pd**	106	107	108 **Ag**	109
							110
							111
							112 **Cd**
125	126	127 **I**	128				
			129				
			130	131	132	133 **Cs**	134
							135
							136
							137 **Ba**
							138 **La**
							139
							140 **Ce**
							141 **Pr**
							142
							143
							144 **Nd**
							145
							146
							147
							148
							149
							150 **Sm**
							151 **Eu**
							152
							153
							154
							155

Table II.

9 **Be**	10 **Ne**	11 **B**	12 **C**	13	14 **N**	15	16 **O**
25	26	27 **Al**	28 **Si**	29	30	31 **P**	32 **S**
41	42 **Kr**	43 **Sc**	44 45 46 47 48 **Ti**	49	50	51 **Vd**	52 **Cr**
67	68 **X**	69 **Ga**	70 71 72 **Ge** 73	74	75 **As**	76	77 **Se**
88	89	90 **Yt**	91 **Zr** 92	93	94	95 **Nb**	96 **Mo**
113	114	115 **In**	116 117 **Vc** (?) 118 **Sn** 119 120	121 **Sb**	122	123	124 **Te**

							156 **Gd** 157 158 159 160 161 162 163 **Tb** 164 165 166 **Er** 167 168 169 170 **Tu**
185	186	187	188 189 190 191 **Os** 192 193 **Ir** 194 **Pt**	195	196	197 **Au**	198 199 200 **Hg** 201
212	213	214	215 216 217 218 219 220	221	222	223	224 225 226 227 228

In the fourth period, a similar step down in the fourth column from iron to cobalt and nickel is necessary to bring copper into an appropriate position; and for a like reason zinc is included in a group with copper. There are similar drops in the eleventh and twelfth columns. Selenium appears with 77 instead of 79 as its atomic weight, in order to preserve it in the oxygen-sulphur series: bearing in mind that the atomic weight of sulphur differs from that of chlorine by fully three units, whereas that of selenium is ordinarily supposed to differ

171	172	173 **Yb**	174 175 176 177 178 179 180	181	182	183 **Ta**	184 **W**
202	203	204 **Tl**	205 206 207 **Pb**	208 **Bi**	209	210	211
229	230	231	232 **Th** 233 234 235 236	237	238	239	240 **U**

from that of bromine by only a single unit, the change does not appear to be an unjustifiable one.

The same argument applies to tellurium in the following period. Personally, I entertain no doubt that the atomic weight of this element will ultimately prove to be considerably lower than that of iodine.

It is unnecessary to go into further details—the table is self-explanatory; but it may not be superfluous to point out that the characteristic

and most remarkable feature brought out by this mode of classifying the elements is the existence in various parts of the system of groups or series of related elements from the highest term of which alone "progression" takes place. The existence of such groups in the case of the iron and platinum metals and some of the rare earths, has long been recognised—but not their significance. It is not unlikely that some of the columns will be "cleared" of such groups when atomic weights generally are known with a closer approach to certainty. Of their existence in column 4, there can be no doubt; but if in column 7 rubidium were put at 84, 85 could be transferred to column 8; and if lower down in the same column, cæsium were put at 131, 132 and 133 could be transferred to column 8, or the necessary "correction" might be made perhaps with greater advantage by including in column 4 two terms (81 and 82) in the fifth, and three (128, 129, 130) in the seventh period: column 7 would then be free from "grouped" elements. Such groups, however, are undoubtedly as characteristic of column 8 as of column 4; whether they are of column 9 is open to question—the cerium group might well follow barium in column 8; but wherever they may come, it is clear that the elements of this group are a very numerous body, and that a remarkable expansion may be looked forward to in this part of the table. Column 11 might also, by a similar process, be cleared of grouped elements. If such a clearance turn out to be possible, grouped elements will be characteristic of only three of the families—those in columns 4, 8 and 12.

Making allowances in the manner suggested, the "smoothed" scheme is arrived at which is embodied in Table II.

The possibilities disclosed by a system of classification such as that here suggested are remarkable, but they are on the surface and need not be dwelt upon. Speculation on such a subject will be justified if it but lead to further appreciation of the rhythm which undoubtedly underlies the relationships subsisting among the elements. That work in plenty is left for the chemist to do is certain.

"Persulphuric Acids." By Henry E. Armstrong, V.P.R.S., and T. Martin Lowry, D.Sc. Received March 13,—Read March 20, 1902.

Although it was observed by Faraday in 1834 that "if the acid were very strong, a remarkable disappearance of oxygen took place" on electrolysing aqueous solutions of sulphuric acid,* it was not until 1878 that its disappearance was at all satisfactorily accounted for by the discovery of *persulphuric acid* by Berthelot.

* 'Researches in Electricity,' series vii, § 728.

Only the anhydride, S_2O_7, was isolated by Berthelot, but he concluded that the corresponding acid was formed (1) on dissolving the anhydride in water, (2) on electrolysing strong solutions of sulphuric acid, and (3) by the interaction of hydrogen peroxide and ordinary sulphuric acid. The correctness of these conclusions appears to have been regarded as beyond question after Marshall had discovered in 1891 that well-defined salts of "Berthelot's acid" could be prepared by electrolysing solutions of potassium or ammonium hydrogen sulphate. Doubt arose, however, when Caro, in 1898, discovered that if Marshall's salts were acted on by sulphuric acid, a new acid was obtained having properties markedly different from those associated with Berthelot's acid. "Caro's acid" soon acquired importance as an oxidising agent, owing to the good use that was made of it by Bamberger and by von Baeyer and Villiger.

Having found that Caro's acid liberated iodine very rapidly whereas Berthelot's acid acted but slowly on iodides, these latter chemists were able to devise a process by which the one acid could be estimated in presence of the other. By removing sulphuric acid by means of barium phosphate, they obtained a solution containing Caro's acid and Berthelot's acid, in which they determined the amount of each of these substances as well as the amount of sulphate to which the persulphuric acids gave rise when decomposed. Their results led them to conclude that the ratio of sulphur to active oxygen in Caro's acid was

$$SO_3 : O = 1 : 1.$$

Placing the simplest possible interpretation upon this result, they assigned to the acid the formula—

$$H_2SO_5 = H_2O_2{\cdot}SO_3.*$$

Meanwhile, the problem had been studied from a somewhat different point of view by Lowry and West,† who had determined the equilibrium subsisting between hydrogen peroxide and "persulphuric acid" in presence of sulphuric acid and water, and had found that the ratio which the hydrogen peroxide bore to total "persulphuric acid" was entirely dependent on the ratio which the water bore to the sulphuric acid—the ratio of hydrogen peroxide to "persulphuric acid" being ultimately the same in a mixture prepared from hydrogen peroxide and sulphuric acid as in a solution of equal strength prepared by electrolysis. The experimental curve approximated very closely to a curve deduced from an equation of the fourth order, and assuming that the chief product of interaction was a persulphuric acid of the series $H_2O_2{\cdot}nSO_3$, it was to be supposed that it was the fourth term of the

* 'Ber. Deut. Chem. Ges.,' 1901, 853.

† 'Chem. Soc. Trans.,' 1900, 950.

series, viz., $H_2O_2 \cdot 4SO_3$. But there were indications that some simpler acid was also present in small quantity.

These results applied mainly to concentrated solutions, 85 per cent. of the change taking place between the limits expressed by the formula $H_2SO_4 \cdot H_2O$ and $H_2SO_4 \cdot 4H_2O$; and virtually no peroxidation of the sulphuric acid took place below the limit expressed by the formula $H_2SO_4 \cdot 6H_2O$. Von Baeyer and Villiger, on the other hand, had dealt with a product existing in dilute solution. The different conclusions deduced from the two sets of results were, therefore, not necessarily discordant; it was possible that the product examined by Baeyer and Villiger had been formed by hydrolysis from the "higher" acid which Lowry and West's observations had indicated was present in concentrated solutions. It is also to be noted that the determination of the ratio of active oxygen to sulphur is sufficient to determine the composition of the acid only in the case of the acid being one of the $H_2O_2 \cdot nSO_3$ series; obviously, other types of "persulphuric acid" are possible.

If we consider what must be the properties of the persulphuric acids generally, it is clear that whereas the salt of a dibasic acid of the formula H_2SO_5 would remain neutral on withdrawal of the peroxide oxygen, salts of higher acids would yield more or less sulphuric acid when decomposed. As a carefully neutralised solution of Caro's acid becomes acid when heated, the salt originally present in it cannot be one derived directly from the acid H_2SO_5—assuming that this is dibasic.

We are much indebted to Mr. A. J. Cook for having made a long series of determinations which show that the ratio of the increase in acidity to active oxygen lost is

$$SO_3 : O = 1 : 2,$$

a result which finds expression in the formula $H_2S_2O_9$ but is in direc opposition to the formula H_2SO_5; thus, supposing the calcium salt to be used:

$$2CaSO_5 = 2CaSO_4 + O_2$$

$$CaS_2O_9 + H_2O = CaSO_4 + H_2SO_4 + O_2.$$

The probability that the acids in question have the composition suggested is considerable, if the manner in which sulphuric acid may be expected to undergo electrolysis be taken into account. Strong solutions of the acid may be supposed to contain both sulphuric and disulphuric acids, and it may be expected that both would "peroxidise"; on electrolysis the former would give perdi- and the latter pertetra-sulphuric acid, thus—

$HO{\cdot}SO_2{\cdot}OH$	$HO{\cdot}SO_2{\cdot}O$ $HO{\cdot}SO_2{\cdot}\dot{O}$.
Sulphuric acid.	Perdisulphuric acid.
$HO{\cdot}SO_2{\cdot}O{\cdot}SO_2{\cdot}OH$	$HO{\cdot}SO_2{\cdot}O{\cdot}SO_2{\cdot}O$ $HO{\cdot}SO_2{\cdot}O{\cdot}SO_2{\cdot}\dot{O}$
Disulphuric acid.	Pertetrasulphuric acid.

An acid of the formula $H_2S_2O_9$ may be regarded as the anhydro-acid derived from permonosulphuric acid, *i.e.*, as $\left.\begin{matrix} HO{\cdot}O{\cdot}SO_2 \\ HO\ O{\cdot}SO_2 \end{matrix}\right\} O$. It is scarcely necessary to say that the interpretation now put upon the data afforded by solutions of Caro's acid will need to be verified, and cannot be accepted as final until salts of the acid have been isolated. This we are engaged in doing.

Further consideration, in the light of the facts brought forward by Baeyer and Villiger and in the present communication, of the results arrived at by Lowry and West, has served only to confirm the conclusion that the chief product of oxidation of sulphuric acid by hydrogen peroxide in presence of less than 50 per cent. of water is an acid richer in sulphur trioxide than perdisulphuric acid, and to justify the assumption on which they based the formula $H_2S_4O_{14}$—namely, that the acid is a member of the series $H_2O_2{\cdot}nSO_3$. Lowry and West determined only the ratio of peroxide oxygen to persulphuric oxygen, and paid no attention to the total oxidising power of the solution. That they were justified in this course is apparent from the fact that no change in the ratio of the two forms of oxygen was observed on varying the strength of the peroxide solution from 10 to 40 "volumes," nor was any change observed as the oxidising power gradually decreased when the acid decomposed.

If the main product of the interaction had been an acid of the formula $H_2S_2O_9$, this would not have been the case, as the equilibrium represented by the equation

$$\underset{C_2}{2H_2O_2} + \underset{C_3}{2H_2SO_4} = \underset{C_1}{H_2S_2O_9} + \underset{C_4}{3H_2O}$$

would depend on the total oxidising power of the solution as well as on the proportion of sulphuric acid and water; this is seen most clearly when the equation of mass action,

$$KC_2{}^2C_3{}^2 = C_1C_4{}^3,$$

is transposed into the form

$$\frac{C_1}{C_2} = K\left(\frac{C_3}{C_4}\right)^2 \cdot \frac{C_2}{C_4},$$

which indicates that the ratio C_1/C_2 of persulphuric oxygen to peroxide oxygen depends not only on the ratio C_3/C_4 of sulphuric acid to water and on the concentration C_4 of the water in the solution, but also on the actual concentration C_2 of the hydrogen peroxide. The fact that the equilibrium is independent of total oxidising power can only be explained if the chief products of interaction are members of the series $H_2O_2 \cdot nSO_3$, or hydrates thereof.

As there was no sufficient evidence to justify the assumption that a third persulphuric acid was present in the solutions they examined, Lowry and West had no alternative but to regard Caro's acid as pertetrasulphuric acid, and their simpler member of the series as the acid corresponding to Marshall's salts. The determination of the ratio of sulphur to active oxygen by Baeyer and Villiger has rendered such a limitation impracticable, and we now feel not only that it is justifiable but that we are compelled to postulate the existence of *at least* three persulphuric acids, viz. :—

Pertetrasulphuric acid	$SO_3 : O = 4 : 1$
Perdisulphuric acid	$SO_3 : O = 2 : 1$
Peranhydrosulphuric acid (Caro's acid)	$SO_3 : O = 1 : 1$

In carrying out his experiments, Mr. Cook nearly neutralised solutions of Caro's acid by means of a carbonate and then neutralised the liquid by means of either sulphuric acid or caustic soda, portions being taken out and tested with methyl orange. Measured portions of the neutral solution were run as quickly as possible into flasks containing a little dilute sulphuric acid to arrest decomposition; the persulphuric acids were then estimated by Baeyer and Villiger's method. Portions of the same solution were heated at 100° until all oxidising power was lost, and the acidity developed was estimated by caustic soda, using methyl orange as indicator.

Solution I.—This was prepared by digesting potassium persulphate at 60—70° with a solution containing only 10 per cent. of sulphuric acid. At the end of about 2 hours the cooled solution was diluted and neutralised with chalk. The amount of iodine liberated at once by the Caro's acid present, expressed in terms of decinormal thiosulphate solution, was equivalent to 41·54 c.c.; the amount of iodine liberated slowly by the perdisulphuric acid present was equivalent to 1·05 c.c. The acid liberated on warming the solution was equivalent to 22·06 c.c. The calculated amount, assuming the ratio $2O : SO_3$, would be $1{\cdot}05 + \frac{1}{2}41{\cdot}54 = 21{\cdot}82$ c.c.

In the subsequent experiments 15 grammes of potassium persulphate was digested with from 35 to 25 c.c. of concentrated sulphuric acid at the ordinary temperature during 1—2 hours. The solution was then diluted with ice and neutralised: in Experiments 2, 3, 4 and 6 with chalk, in 5 with sodium bicarbonate, and in 7 with sodium carbonate,

using phenol phthalein as indicator. In Experiment 7 the solution was mixed with excess of caustic soda before warming it to decompose the per-salts, and the excess was subsequently determined. No hydrogen peroxide was present except in solution 5, in which case the amount was determined by permanganate and allowed for.

The following are the results obtained in terms of decinormal solutions :—

	Caro's acid.	Perdisulphuric acid.	Acid liberated. Found.	Acid liberated. Calculated.
I	41·54	1·05	22·1	21·8
II	38·8	5·91	26·6	25·3
III	35·0	2·77	20·2	20·3
IV	39·24	2·24	21·3	21·8
V	37·3	1·56	21·5	20·2
VI	49·8	3·43	28·7	28·3
VIII	23·3	0·71	12·2	12·4

"The Conditions determinative of Chemical Change and of Electrical Conduction in Gases, and on the Phenomena of Luminosity." By HENRY E. ARMSTRONG, V.P.R.S. Received March 13,—Read May 1, 1902.

In his communication to the Chemical Society on the union of oxygen and hydrogen, read at the meeting on February 19,* Mr. H. Brereton Baker has added another to his brilliant series of proofs that interactions supposed to take place between two substances are in reality of a more complex character; and having successfully demonstrated, in the case of the gases referred to, that water alone does not determine the interaction, he has, I believe, carried the investigation to the final stage which it was essential it should reach to make it a complete discovery of the nature of the process.

Mr. Baker has shown that when a mixture of hydrogen and oxygen [most carefully prepared by electrolysing a solution of barium hydrate] is enclosed in a chamber of hard Jena glass [cleansed in the most careful manner possible], and the mixture is dried as thoroughly as may be [by means of phosphoric anhydride which has been carefully purified by distilling it in a current of air], no appreciable interaction takes place between the gases, even on heating the tube to redness. If the drying be not carried too far, however, water is gradually but very slowly formed—finally in sufficient quantity to be visible: and yet, even when water is visibly present, no explosion takes place.

The interpretation I would give of these observations is as follows:—

* 'Chem. Soc. Proc.,' 1902, p. 40.

The amount of *impurity* present in the gases being reduced to a minimum, *i.e.*, the gases being *almost* dry and *almost* free from impurities *which in admixture with water constitute a conducting system*, change takes place at a *very* slow rate when heat is applied; and even when a considerable amount of *water* is present, the amount of associated impurity is too small to raise the conductivity—the rate of formation of conducting systems—to a point at which the rate of change would be such as to give rise to an explosive wave.

As defining the conditions which it has long been my opinion are necessary to the occurrence of chemical change in gases and generally, I may refer to words used by me in 1893.* I venture to call attention to them, not because I have any particular wish to put forward a claim on my own behalf but in the hope of attracting attention to a subject of surpassing importance, which both chemists and physicists have hitherto most strangely neglected to consider *in all its bearings.*

* "Eight years ago, in the course of the discussion on Mr. H. B. Baker's communication on "Combustion in Dried Gases" (these 'Proceedings,' 1885, 40), I defined chemical action as *reversed electrolysis:* in other words, in order that chemical action may take place, it is essential that the system operated on comprise an electrolyte. I then pointed out that as neither hydrogen nor oxygen was an electrolyte, a mixture of only these two gases should not be explosive; and, moreover, that as water was not an electrolyte, and it was scarcely probable that water and [either] oxygen or hydrogen would form an electrolyte, it was difficult to understand how the presence of water pure and simple should be of influence in the case of a mixture of hydrogen and oxygen. This forecast has since been verified, the remarkable series of experiments carried out by V. Meyer in conjunction with Krause and Askenasy having clearly demonstrated that the formation of water from hydrogen and oxygen takes place at an irregular rate, and is, therefore, dependent on the presence of a something other than water—I imagine an acid impurity. But this is a consideration which has not yet received the proper attention, and it is, therefore, desirable to emphasise its importance by reference to other cases. Mr. Baker's recent preliminary note on the influence of moisture in promoting chemical action (*ante*, p. 229) affords several interesting examples:—Thus, he states that neither does hydrogen chloride combine with ammonia nor is nitric oxide oxidised by oxygen if moisture be excluded. In the former case, the addition of water should suffice to determine the combination, as water and hydrogen chloride together form a 'composite electrolyte' (*cf.* 'Roy. Soc. Proc.,' 1886, No. 243, p. 268); as neither nitric oxide nor oxygen, however, forms a composite electrolyte with water, in this case water alone should not determine the occurrence of change—but if by the introduction of a trace of 'impurity' in addition to water the presence of a composite electrolyte were secured (however high its resistance, owing to the smallness of the amount of 'impurity'), action would set in, and when once commenced would proceed at an increasing rate, as nitric acid would be formed and the resistance of the electrolyte would consequently diminish. On this account it will be a task of exceeding difficulty to demonstrate experimentally that nitric oxide and oxygen are inactive in presence of water alone; but there can be no doubt that such must eventually be admitted to be the case, provided always that it is permissible to extrapolate Kohlrausch's observations and to conclude from them that *pure* water is a dielectric."—'Chem. Soc. Proc.,' 1893, 145.

[In his complete paper,* Mr. Baker states that he found that hydrogen and oxygen did not interact when subjected to the influence of a coil of thin silver wire heated by an electric current to the melting point of the silver, that is to say, above 1000°; but that when a similar platinum coil was heated in the dried gaseous mixture an explosion occurred just after the wire became visibly red. Doubtless, in the latter case, the surface over which interaction extended was sufficiently large to raise the velocity of change to the explosive rate, owing to the condensing effect exercised by the platinum; and the silver had no effect because it is destitute of the power which platinum possesses in so high a degree of attracting gases and of acting as a catalyst.

In making this statement, I wish it to be understood that I assume that platinum *per se* would be without effect. I feel almost confident, in fact, that if they could be dealt with, even free atoms would not associate in the absence of a composite electrolyte, as it appears to me probable that at least one function of the composite electrolyte is to provide the "mechanism" by means of which the energy of chemical change is frittered away. Ostwald, in his lecture on Catalysis (an English translation of this is to be found in 'Nature,' April 3, 1902), defines a catalyst as any substance which alters the velocity of a chemical action without appearing in the final product. I conceive, however, that the catalyst determines the interchange from its beginning. I have discussed a number of "catalytic" phenomena—including those of fermentation and of the dissolution of metals in nitric acid—in the address referred to later on.—*Note, added May* 2.]

It appears to me that Brereton Baker's results lead to far-reaching consequences—that they justify, not only the conclusions I have drawn as to the conditions which determine the occurrence of chemical change, but also the conclusion already stated by me in the note previously referred to, in 1893, that *pure* gases should be perfect dielectrics: *i.e.*, that the passage of an electric discharge through a gas, like that of an explosive wave through, say, a mixture of hydrogen and oxygen gases, can only take place if an electrolyte be present, such electrolyte being, it would seem, always a composite system, and one which may be pictured as existing momentarily in a quasi-liquid state.

The argument was more fully developed in my Presidential Address to the Chemical Society in 1895, in which I discussed very fully the nature of chemical change, and the conditions which determine it, from various points of view.†

* 'Chem Soc. Trans.,' 1902, p. 400.

† No more striking evidence that the occurrence of a discharge in a gas is dependent on the presence of a something besides the gas—on the formation of a complex conducting system—could be given than is afforded by the beautiful

No attempt has been made as yet to deal practically with the problem of electrical discharge in gases in the way in which Dixon, Brereton Baker and Shenstone have dealt with that of chemical change. Vacuum tubes are invariably made of soft glass—which is altogether unfitted for accurate experiments, as it is always more or less readily attacked by moisture, yielding up traces of alkali; no special care is taken in cleansing them; no special precautions are used in filling them; and both oil of vitriol and *commercial* phosphoric anhydride are used as drying agents, although it is well known that these may be fruitful sources of contamination. And the electrodes offer special difficulties, which it may be impossible to overcome, owing to the readiness with which metals occlude gases.

If there be evidence to show that the discharge in vacuum tubes is conditioned by the presence of impurities, as I contend there is, it is open to question whether most, if not all, of the effects attributed to the so-called radiant matter—or as Sir Wm. Crookes now expresses it, to electrons—are not in reality due to ordinary gross matter. To take an example from this author's recent communication to the Society on "Radio-activity and the Electron Theory," at the close of the paper reference is made to the discharge from a silver pole of *electrons* which cause the glass against which they strike to glow; at the same time, the silver volatilises and is deposited near the pole. "While the volatilisation of the pole is rapidly proceeding, the metal glows as if red hot. This 'red heat' is superficial only." It appears to me possible to give a more ordinary explanation of the remarkable phenomenon thus described by Sir Wm. Crookes. A silver electrode would be more or less "polarised" with oxygen—for we know that silver has the power of absorbing oxygen when fused, and that the gas is only partially extruded as the metal cools; in any case a minute amount of oxygen would be present in the tube. When heated by the discharge *in vacuo* the silver-oxygen "compound" would

experiment exhibited by Dewar at the Royal Institution, showing that the discharge at once ceases in one of Crookes' phosphorescent tubes on cooling with liquid oxygen. Such treatment cannot be supposed to cause the condensation of the residual air, but may well condition the deposition of the traces of vapour (? of conducting water) present in the gas, thereby destroying the systems within which conduction can alone occur.

It may be here pointed out that this argument is perhaps also at least partially applicable in explanation of the erratic behaviour of the discharge in vacuum tubes. If the discharge occur within a complex system, it may well be that certain conditions will favour the formation of a conducting system including one, and others of a system including another, of several substances present in a tube —such an explanation, for example, would account for the fact that the mercury spectrum is only sometimes seen in tubes connected with a mercury pump. And having regard to the fact that a gas may exhibit several spectra, it may be that the spectrum of a given substance varies more or less according as it is included in one or other of several conducting systems.—('Chem. Soc. Trans.,' 1895, 1141.)

dissociate—*i.e.*, oxygen would be given off, but would be in part reabsorbed, so that the silver would never be entirely free from oxygen; the change, in fact, would oscillate about a point of equilibrium, and would never be total in the direction corresponding to the complete deoxidation of the silver. May not the bombardment of the glass described by Sir Wm. Crookes have been effected by the displaced oxygen molecules, and may not the surface heating of the pole have been an outward and visible sign of the return of the truants to their silvery home?

A diamond pole, such as Sir Wm. Crookes speaks of, might behave in a similar manner in presence of a minute amount of oxygen; in fact, the argument applies generally to the occlusion of hydrogen and other gases by electrodes.

An argument which I think will sooner or later be regarded as of weight in favour of the view that the phenomena are electrolytic in their origin is afforded by the luminous manifestations in vacuum tubes. These can scarcely be either mere collision effects, or mere heat effects. It has long seemed to me that luminosity and line spectra are the expression—the visible signs—of the changes attending the formation of molecules from their atoms, or, speaking generally, *that they are consequences of chemical changes*, a chemical change being one which involves an alteration of molecular composition, or it may be of molecular configuration, as it is conceivable that even changes involving but the formation of isodynamic (tautomeric) molecules—changes in molecular structure unattended with change in molecular size—may give rise to such manifestations. Dealing with the question of the luminosity of hydrocarbon flames, I expressed this view in 1895 in the terms given below.*

* "Interest should be revived in the subject by the recent observations of V. B. Lewes ('Roy. Soc. Proc.,' 1895, vol. 57, p. 450) on the decomposition of acetylene at high temperatures, which have led him to suggest that this compound plays the chief part in promoting luminosity in hydrocarbon flames, and that the heat liberated during its decomposition endows the carbon particles produced from it with an incandescence far higher than corresponds to the temperature of the flame. It appears to me that while accepting this as a partial explanation, it is unnecessary to suppose that luminosity is consequent on the production of presumably solid carbon particles; the conversion of acetylene, or other hydrocarbons, at high temperatures into hydrogen and carbon, although a decomposition in the ordinary sense of the term, is doubtless a change involving the interaction of several molecules, and the consequent formation of carbon molecules of a high order of complexity together with hydrogen molecules; and it is exothermic because the energy liberated in the combination of the carbon atoms among themselves, and also of the hydrogen atoms, is far in excess of that absorbed in the decomposition of the hydrocarbon molecules. The initial luminance may therefore well be that of the molecules at the moment of formation prior to condensation, although the continued incandescence of the carbon molecules after their reduction to the solid state probably contributes largely to maintain luminosity, giving to hydrocarbon flames their special value, perhaps. But this is to explain the phenomena in terms

In the discussion on Mr. Campbell Swinton's paper on the luminosity of the rare earths when heated *in vacuo* by means of cathode rays, read in April, 1899,* I applied this view in explanation of the brilliant luminosity of certain rare earths, and also of the electric arc, suggesting in the latter case that the carbon molecules were dissociated by the passage of the current, and that the re-association of the atoms into molecules gave rise to the intensely luminous effect which is characteristic of the arc.

Bunte has argued,† in the case of the Auer von Welsbach gas mantle, that the high luminosity is not so much due to a specific superior radiating power of the earth used as to the fact that, in virtue of the power which a substance such as ceria possesses of forming a peroxide, combustion takes place to a greater extent at a surface on which it is present than on one consisting of a neutral oxide: consequently, the temperature is higher at such a surface. This undoubtedly must be the case; but I would go further, and regard *the chemical changes* occurring at the surface as the direct seat, or origin as it were, of the luminosity. Probably, a higher oxide is alternately decomposed and reformed—in other words, *the process is one of oscillatory or recurrent oxidation.* Owing to the influence which the oxide exercises as a catalyst, combustion doubtless takes place at its surface more completely than it would at a corresponding neutral surface, and in consequence the temperature developed at the oxide surface is above that of the flame generally.

[It is noteworthy that besides ceria, which is by far the most efficient, only one or two other oxides are effective excitants; and that cerium dioxide is remarkably stable at high temperatures.—*Note, added May* 2.]

A similar explanation may be applied to the incandescence of oxides generally, such as is witnessed, for example, in the lime and zirconia lights. The Nernst lamp probably owes its efficiency to a like cause. It may well be that in the case of the incandescent mantle the maximum effect is produced when only a relatively small proportion of active oxide is present, because at the particular state of dilution the oxide is present in solid solution in a form in which it is most prone to suffer change—that besides being placed under the necessary conditions of freedom, it attains to the most suitable degree of molecular complexity, and therefore to its greatest activity, in dilute solid solution just as many substances do in dilute liquid solutions.

Mr. Campbell Swinton's observations appear to me to be in harmony

of Frankland's theory, as I understand it, and as appears to be necessary in order to explain the luminous appearance of a hydrogen-oxygen flame burning under pressure, and other cases in which the products of combustion are gaseous."—'Chem. Soc. Trans.,' 1895, p. 1147.

* 'Roy. Soc. Proc.,' vol. 65, p. 115.

† 'Deut. Chem. Ges. Ber.,' 1898, p. 5.

with these conclusions. An intensity of cathode rays that gave a brilliant light with "pure" thoria and with a mixture of 99 per cent. thoria with 1 per cent. ceria was found by him to give practically no light with "pure" ceria and with a mixture of equal parts of thoria and ceria. It is improbable that Mr. Swinton dealt with *pure* thoria, as the methods of purification practised at that time did not suffice to give a pure product. That thoria alone gave more light under the influence of cathode rays than in the Bunsen flame may have been due to the fact that it was more intensely heated in the former case, the effect produced by minute proportions of the exciting oxide being much dependent on temperature, and greater the higher the temperature.

The need of an explanation such as is here given of the luminosity of oxides at high temperatures has, I know, occurred to others. My colleague, Professor Ayrton, has long held such a view, and he informs me that, in the course of conversation with him in 1897, Professor Elihu Thomson gave it as his opinion that the brilliant incandescence of lime in the oxy-hydrogen flame was not a mere high temperature effect.*

The phenomena of phosphorescence under the influence of the electric discharge exhibited by the oxides and basic sulphates of the rare earths, with which Sir Wm. Crookes's classical researches have made us familiar, may be included in the same category with those above discussed: if not due to recurrent oxidation, they may be cases of recurrent polymerisation. Apparently the same oxides act as excitants whether a flame or cathode rays be used. Sir Wm. Crookes, it is well known, holds the view that yttria, lanthana, &c., are characterised by definite phosphorescent spectra; on the other hand, Lecoq de Boisbaudran has contended that the yttria and gadolinite earths are not self-luminous. This latter view has recently been confirmed by Baur and Marc.† According to these observers, whilst the colourless oxides and salts of yttrium, gadolinium, and lanthanum are not specifically luminescent, if mixed with minute proportions of the earths which have coloured salts, viz., erbium, neo- or praseodymium, they afford spectra such as have hitherto been regarded as characteristic of themselves; and such spectra may be equally well developed by using lime or calcium sulphate as diluents; yttria itself, however, fails to give a spectrum when mixed in small proportion with lime.

[If it should eventually be established that the emission spectra are conditioned solely by "earths" which afford coloured salts, as the emission spectra appear to be closely related to the absorption spectra, dark lines in the one taking the place of bright lines in the other, it

* Comp. 'Journal of the Society of Arts,' Feb. 10, 1899, p. 256; E. Pringsheim, International Physical Congress at Paris, Report 2.

† 'Deut. Chem. Ges. Ber.,' 1901, p. 2460.

will be possible to correlate the production of visible colour with that of a luminous spectrum, and to regard both as originating in the same intramolecular mechanism.—*Note, added May* 2.]

The argument may be carried a stage further and applied to phosphorescent phenomena generally. It appears possible to regard all these as the outcome of oscillatory changes in molecular structure—and the slow decay of the effect which is observed in many cases may be compared with the slow discharge of a condenser—the charging of which be it noted may be regarded as but a process of molecular deformation of the dielectric.

In a case like that of radium, it would seem that energy of low period suffices to bring about the change the reversal of which subsequently gives rise to the luminous effect and radio-activity. The chemist is tempted to contrast the behaviour of radium with that of a substance undergoing change into an isodynamic form—such as nitro-camphor, for example. Nitro-camphor $C_8H_{14}<\begin{matrix}CH.NO_2\\ \downarrow\\ CO\end{matrix}$ dissolves readily in alkalies, but its salts are derived from a pseudonitro-camphor, the group $CH.NO_2$ undergoing alteration into the group $C(NO.OH)$. When liberated from its salts, the pseudo-form at once, in great part, reverts to the normal form; on the other hand, when solid nitro-camphor, which apparently consists almost entirely of the normal form, is dissolved in a liquid, it in part gradually undergoes conversion into the pseudo-form. A point of equilibrium is eventually reached when from 7 to 10 per cent. of the pseudo-form is present.* The change proceeds at different rates in different solvents, and is undoubtedly conditioned by some catalyst: in other words, either form of the *pure* substance would be stable. It will be apparent that the change is one which does not affect the molecule as a whole, but merely an isolated region in it. If the energy set free in the formation of the dominant form were of sufficiently high period, nitro-camphor might appear luminous and even be radio-active. The argument is of interest, as it serves to suggest that radio-activity may not be the isolated phenomenon we at present suppose but a concomitant of some chemical changes.

[From this point of view, it appeared to be desirable to examine substances such as saccharin (orthobenzoic sulphinide) and cane-sugar, which glow in a very remarkable manner when broken up. Mr. E. W. Lewis has kindly made a number of experiments for me, but without obtaining positive results: although an intense photographic impression of the flash is easily obtained by merely crushing crystals of saccharin or sugar on a glass plate held above a sensitive film, crystals placed on a film are without effect, even when left in contact with it during several days. It will, however, be desirable to

* Comp. Lowry, 'Chem. Soc. Trans.,' 1898, p. 966.

apply the far more delicate electroscopic test to such substances under various conditions. It so happens that the property referred to was first noticed in the case of saccharin in my laboratory (in 1895) by Mr. Wm. Jackson Pope,* and in March last I applied to this gentleman for a specimen of the substance he had prepared in 1895. He was able to send me crystals, but informed me that those he had tested were inactive. We found this to be true of the remainder, seventeen out of thirty-six crystals being inactive, at most a very feeble glow being observed on crushing the others; but active crystals were obtained on recrystallising the inactive material from acetone. Old specimens of sugar crystals appear to be as active as new ones.—*Note, added May* 2.]

A class of effects attendant on radio-activity, and which have an important bearing on the question previously considered, are those attributed to emanations from radio-active substances. Sir Wm. Crookes has brought the subject prominently under notice in his recent communication. The behaviour of such emanations is strikingly similar to that of ordinary gross matter; indeed, it is difficult to resist the conclusion that such is their character, and that they are but secondary products engendered by radio-activity.

[Professor Rutherford's recent observations on the "emanation" from thorium compounds are very difficult to interpret. The assumption that Mr. Soddy and he are inclined to make,† that the emanation is allied in properties to the elements of the argon group is almost a contradiction in terms. Such a constituent presumably would be an inert substance, and if not removed by the drastic treatment to which they submitted their material without permanently affecting its radiative power, it would scarcely then escape spontaneously on mere exposure of the solid. It would seem to be far more probable that the "emanation" is a secondary manifestation, in some way conditioned by the "straight-line radiation."

There is a feature in Dr. Russell's experiments which has always struck me as remarkable, assuming that the photographic effects he has obtained are due to hydrogen peroxide, viz., the absolute sharpness of the images formed by a scratched zinc plate even when this is placed at a relatively considerable distance above the sensitive film. If the emanation passed by mere diffusion from the active surface, a more or less blurred image might be expected to form. It would almost seem that the molecules are projected in straight lines—in other words, that they may be electrically charged. If so, a distinction should perhaps be drawn between a merely vaporised substance and a "nascent" substance.—*Note, added May* 2.]

The production of hydrogen peroxide under such an influence at

* 'Chem. Soc. Trans.,' 1895, p. 985.

† 'Chem. Soc. Trans.,' 1902, p. 321.

once presents itself not merely as a possibility, but practically as a necessity. There is every reason to suppose that hydrogen peroxide is a necessary product of oxidation by ordinary oxygen whatever the substance oxidised; but the conditions under which it is produced are usually such as to engender its own immediate decomposition—so that it either escapes observation or is met with only in minute quantity. The oxidation of zinc may be taken as an example. The oxidation necessarily takes place in a circuit comprising (*a*) the impure metal—which because it is impure furnishes both negative and positive electrode—(*b*) conducting (impure or dirty) water and (*c*) oxygen, of which the last acts as depolariser. The water is electrolysed, its oxygen going to the metal; at the same time, the hydrogen from the water becomes associated with the depolarising oxygen *molecule*, forming hydrogen peroxide, H_2O_2: the products, in fact, are zinc oxide *and hydrogen peroxide* in equivalent quantities, not merely zinc oxide as is taught by the text-books.

It does not seem to me that Sir Wm. Crookes has disproved the possibility that the effects may be due to hydrogen peroxide by his experiment in which one limb of a U-tube passed through the cork in a bottle containing a solution of the peroxide, the open end of the other limb being put close to a sensitive film, the result being that no photographic effect was observed in 72 hours, although a strong effect was produced when the film was kept over the mouth of the bottle during only 24 hours. The peroxide molecules would undoubtedly tend towards the walls of the tube, especially at the bend; but collision therewith would probably have fatal effects, as the alkaline surface of the glass would promote their destruction.

The argument may be applied to recent researches on the so-called ionisation of air and other gases. If this term implied the recognition of nothing more than the power of conducting, little difficulty would arise. Faraday, with characteristic and truly scientific caution, used the word *ions* simply "to express those bodies which can pass to the electrodes." Unfortunately, of late years the signification of the word has been entirely altered: ionisation now connotes a schism—a process of molecular suicide; and the ions are looked upon as discrete particles enjoying separate existence, as the electrically charged, wandering elements of molecular disruption. Before we assume that such ions are present in air under ordinary conditions, and that they alone can condition conductivity, it behoves us to examine carefully into the nature of the evidence which is relied on in proof of their presence. It appears to me that much of the work done in this field is open to criticism. Thus, in Mr. C. T. R. Wilson's experiments,* even the minute amount of chemical change occurring at the surface of the sulphur bead attaching the gold leaf of the electroscope to its

* 'Roy. Soc. Proc.,' 1901, vol. 68, p. 151; vol. 69, 277.

support may have sufficed to render the atmosphere within the flask conducting. And in the later experiments of the same observer, the phosphoric anhydride used as drying agent may have contributed an acid impurity. Sir Wm. Crookes has recently given an illustration of the manner in which impurities may be derived from phosphoric anhydride. By heating it strongly *in vacuo*, he got rid of these to such an extent that they were no longer recognisable by the spectroscope; but it cannot be supposed that even such treatment suffices to render the anhydride innocuous, bearing in mind the extraordinary delicacy of the electroscopic test.

I venture to think that until the phenomena of conductivity presented by gases have been studied not merely with the same, but even with far greater, care than has been devoted to the study of those attending gross chemical changes in gases, it is premature to conclude that gases undergo ionisation—using the word in its modern sense. I also venture to think that the question whether mere molecules cannot form conducting systems has not yet received in any way the attention it deserves from those engaged in these inquiries.

"On the Properties of the Arterial and Venous Walls."* By J. A. MacWilliam, M.D., Regius Professor of Physiology in the University of Aberdeen. Communicated by Sir M. Foster, Sec. R.S. Received October 24,—Read November 28, 1901.

(From the Physiological Laboratory of the University of Aberdeen.)

Post-mortem *Contraction of Arteries.*

So long ago as 1891 I noticed that the excised carotid artery of a recently-killed ox exhibits a strongly-marked and long-persisting contraction of its muscular wall. The phenomenon is, of course, most easily studied in the arteries of the larger animals. In this investigation I have examined arteries from thirty-five oxen and a considerable number of horses, sheep, cats, and men.

When the carotid of the ox or horse is exposed immediately after death, it is found to be soft and flaccid, and more or less flattened in section, with a large bore, usually 5—6 mm. On exposure to the air, cooling, manipulation, cutting, &c., the artery speedily becomes rigid and contracted; the tube becomes circular in section, and its calibre is greatly reduced, *e.g.*, to an internal diameter of 2—3 mm., or less. The artery becomes so stiff that a piece 7 or 8 cm. long may

* A statement of the chief conclusions arrived at in this investigation was communicated to the Physiological Society (at Edinburgh) on July 20, 1901.

often be held out (by one end) in an almost horizontal position. These changes may be seen 10—15 minutes after death or many hours later; they occur in all systemic and pulmonary arteries that have well-developed muscular coats. An artery excised shortly after death may be strongly and persistently contracted long before *rigor mortis* has appeared in the heart or skeletal muscles, and indeed while these muscles are still obviously living.

Causes of Post-mortem *Contraction.*

(1.) Mechanical stimulation has a powerful influence, *e.g.*, cutting, manipulation, &c. When an artery is cut across, contraction begins at the cut, and thence spreads along the tube more or less completely; an excised piece of artery some inches long often shows very well-marked contraction near both ends, while the middle part may be relatively soft and relaxed; an incision made in this middle part speedily induces marked contraction there also.

Pieces of excised artery show considerable variation in the extent to which the contraction excited at the cut ends involves the intermediate portion of the tube.

(2.) Cooling to a few degrees above zero favours the development of contraction in an exposed artery. Warming a contracted (cold) artery up to about 40° very commonly has a markedly relaxing effect, though the relaxation is usually incomplete; a piece of contracted artery put into defibrinated blood and kept in a warm chamber for an hour or so usually relaxes to a considerable extent—at least, if the experiment is done within a day or so after death, the artery being excised shortly after death; it may again contract very markedly when the temperature falls.

(3.) Exposure to the air also seems to play a part in inducing and favouring *post-mortem* contraction.*

When a piece of artery is excised as speedily as possible after death and at once immersed in olive oil while still flaccid, and kept in the oil, *post-mortem* contraction is, as a rule, much less strongly marked, though it may be extremely long-continued.

Again, when a relaxed artery is exposed immediately after death and covered over with olive oil, cutting into the arterial tube while it is immersed in oil causes, as a rule, a decidedly less pronounced contraction than usual.

Post-mortem contraction is well marked in all arteries, pulmonic as well as systemic.

In the lungs of an ox an hour after death I found, on making deep

* In the case of a living artery *in situ* (posterior tibial), John Hunter observed that the vessel contracted very much on being exposed to the air for some time. Cooling may possibly have played some part here. 'Works' (London, 1837), vol. 3, p. 157.

incisions, branches of the pulmonary artery uncontracted and flaccid, but on excising portions these speedily became contracted, so that a piece of artery 4 cm. or more in length could be held out in a horizontal position by one end. Later—3 hours after death—deep incisions into the lung substance showed the small arteries to be markedly contracted and firm, their cut ends projecting somewhat on the surface of the incision. Branches of the pulmonary artery 3 mm. in diameter when relaxed were found to be less than 2 mm. when contracted.

Small branches arising from the carotid are often apparently more firmly contracted than the carotid itself.

Arteries left undisturbed *in situ* for 24—48 hours after death, and then exposed, may show little sign of contraction at first, but under the influence of manipulation, cutting, exposure to air, &c., may soon enter into strong contraction—persisting for days.

The importance of the presence or absence of *post-mortem* contraction need hardly be insisted on with regard to the measurements of arteries made after death to ascertain the size of lumen, the thickness of wall, tunica media, &c.

Changes in Length of Empty Excised Artery in Contraction and Relaxation.

When an empty excised artery contracts its length becomes increased, and conversely on relaxing the tube shortens. Thus two portions of artery (ox), measuring 25 mm. and 31 mm. in length, while strongly contracted became shortened to 20 mm. and 27 mm. respectively when the muscular contraction had relaxed to a great extent.

Duration of Post-mortem *Contraction.*

In the arteries of the ox and horse *post-mortem* contraction usually lasts for several days; the usual duration of obvious contraction is about 5 or 6 days, when the artery is kept in an ordinary room. In a warm room contraction passes off earlier, and in a cool room its duration is considerably prolonged. When placed in a warm chamber (in defibrinated blood) at about 40° C., the contracted artery is found to become completely relaxed in 24 hours or less; its walls become flaccid, and its lumen enlarges from a diameter of perhaps 2 or 3 mm. to one of 5 or 6 mm. Subsequent stimulation (mechanical, chemical, electrical, &c.), cooling, and exposure to air, entirely fail to induce any contraction. There is no evident change in reaction to litmus paper.

Immersion in olive oil exercises a powerful influence in prolonging the duration of *post-mortem* contraction. In the carotid of the horse so treated I have seen an appreciable amount of contraction present as

long as a fortnight after death; a very slight residuum of contraction has been found even 18 days after death.

Persistence of Excitability in Arteries after Death.

The excised artery of a healthy animal kept at ordinary room temperature either in defibrinated blood or simply in a bottle moistened with salt solution, commonly reacts to certain modes of stimulation for very many hours after death, often for 2 or 3 days. This is very clearly evident when one makes a transverse incision across an artery which has not shown much contraction after death, but has remained with only a slight development of rigidity in its walls and with a relatively large lumen (*e.g.*, 4—5 mm.); soon after the arterial tube is cut across contraction begins—slowly and gradually—to manifest itself, the walls of the vessel become firm, and the lumen diminishes, so that in 5 minutes the diameter may be reduced to about one-half (2—3 mm.). In such cases decided contraction is usually evident in a minute or two after the cut has been made, though it does not attain its full development for some little time.

Again, it is often seen that when an artery has gone into the contracted state soon after death, it may relax to some extent in a day or two, and then a fresh incision, 2 or 3 days after death, may lead to well-marked contraction. This I have observed most frequently in the arteries of the horse.

Gentle manipulation of the artery with the fingers also tends to promote the development of the contraction. Strong galvanic currents induce contraction. Faradic currents, unless very strong, have little or no effect a day or two after death, when other forms of stimulation are quite effective.

Exposure to chloroform vapour is a powerful agency for causing contraction. This is readily demonstrated by putting a piece of artery into a tightly corked or stoppered bottle containing a little chloroform, the artery being supported in such a position as to obviate any contact with the liquid chloroform while it is freely exposed to the vapour. When so treated, an artery which is only slightly and partially contracted a day or two after death shows a very striking change. Its walls soon become firm, while its lumen becomes greatly contracted, *e.g.*, from a diameter of 4—5 mm. to one of 1·5 or 2 mm.; the surface of the artery assumes a whitish appearance.

The contraction so induced may persist for a long time or it may diminish appreciably in an hour or two. In the latter case a fresh exposure to chloroform vapour may again cause contraction. Much depends on the length of time the chloroform vapour is allowed to act; prolonged action of chloroform induces a permanent change in the arterial wall.

Even so long as 4 days after death the carotid of a horse was seen to contract from a diameter of 5 mm. to one of 3 mm.

Suprarenal extract has a marked influence in inducing arterial contraction even at relatively long periods after the death of the animal, *e.g.*, 24—48 hours, or longer. An excised portion of artery placed in a watery decoction (made with normal saline) of suprarenal medulla speedily goes into strong contraction, persisting for a day or two; at length the contraction passes off. As might be expected the contraction is excited more effectively in a relatively thin-walled artery like the carotid of the sheep than in the thick-walled artery of the ox or horse. Decoctions of suprarenal medulla made with tap water, as well as with normal saline, were tried.

It is to be noted that both tap-water and normal saline, by themselves, have a very appreciable effect in inducing contraction in an artery for some time after death. But these effects are much more transient than those produced by suprarenal extract; the contraction following the application of normal saline or water commonly passes off within 2 or 3 hours, while that induced by suprarenal extract is usually well marked till the following day or even later.

When a certain period, varying according to circumstances, has elapsed after death, an excised artery, though still showing marked contraction, fails to respond appreciably to any form of stimulation.

Prevention of Post-mortem *Contraction* (*by Freezing*).

Freezing a piece of artery immediately after excision from a recently-killed animal prevents the development of contraction altogether if the artery be taken out speedily while still flaccid and at once frozen for a sufficient time. When the artery is at length allowed to thaw it remains permanently relaxed, with large bore (5—6 mm). To bring about this result, 4 hours' freezing suffices in all cases; very often much shorter periods ($\frac{1}{2}$, 1, 2, 3 hours) are enough. In these experiments care must be taken to prevent any of the mixture gaining access to the artery. Arteries so treated respond to no form of stimulation. There seems to be no appreciable change in the reaction of the cut surface of an artery (to litmus paper) after freezing.

There is some evidence tending to show that pieces of artery taken very early after death may be prevented from going into contraction by shorter periods of freezing (*e.g.*, 30 min., &c.) than is necessary with pieces taken later.

Freezing one end of a segment of uncontracted artery (excised immediately after death) for some hours causes that end to remain large in calibre and relatively flaccid, while the other end which has not been frozen goes into marked contraction and usually remains contracted for days.

Periods of freezing insufficient to entirely obviate the appearance of *post-mortem* contraction may cause it to be much less pronounced.

When a portion of artery is rapidly excised immediately after death and quickly cooled to about 0° (while still uncontracted) in a suitable metal vessel placed in ice for some hours, the onset of contraction is usually delayed though not ultimately prevented; in some cases its development seems to be very imperfect until the artery is stimulated (manipulation or cutting, &c.).

When contraction has been established for some time (*e.g.*, a day or two) cooling down nearly to 0° for some hours has no appreciable effect.

Removal of Post-mortem *Contraction.*

Freezing.—An artery, however firmly it may be contracted, can be made to relax by freezing it for some hours in the way already mentioned as being effective in preventing the development of contraction. The exact time required varies in different cases, but I have always found 4—5 hours sufficient. On being allowed to thaw, the lumen enlarges to the usual size of the passive artery, while its walls become soft and relaxed. The contractility of the artery is completely and permanently abolished. Its length is less than in the contracted state. There is no apparent change in reaction of the cut surface to litmus paper.

It often seems to require a less prolonged period of freezing to obviate the appearance of contraction in an uncontracted artery than to remove contraction when it has been established for some little time (1 hour, &c.).

Sulphocyanide.—Immersion of a contracted artery in a solution of sulphocyanide of potassium soon leads to complete relaxation. When a 20 per cent. solution is used, relaxation is generally found to have occurred in 20—30 minutes, the time varying with the thickness of the arterial wall, &c. Weaker solutions require longer time. Sulphocyanide of ammonium has effects similar to the potassium salt. The effect may be graphically recorded in the following way:—A strip of arterial wall cut transversely to the long axis of the vessel is suspended in a bath of the solution and made to pull upon a recording lever, which is kept in a horizontal position by a long, feeble, spiral spring. The tracing obtained shows the extensive relaxation which occurs after the sulphocyanide is introduced into the beaker. A skeletal muscle placed alongside the arterial strip in the bath and connected with a similar lever shows a striking contrast, contracting strongly under the influence of potassium sulphocyanide, as Kühne* described many years ago. Here we have the remarkable result that while potassium sulphocyanide induces strong and persistent contraction in

* 'Myologische Untersuchungen,' p. 130.

skeletal muscle, it (or the ammonium salt) causes speedy and complete relaxation of the contracted arterial muscle (fig. 1). This relaxing effect is much more powerful and rapid than that which follows immersion in saline solutions, such as ammonium chloride (13 per cent.), which dissolve out the muscle-proteid, and the mode of action seems to be different, for the sulphocyanide solution seems to extract very little proteid from the arterial wall when it has acted sufficiently long

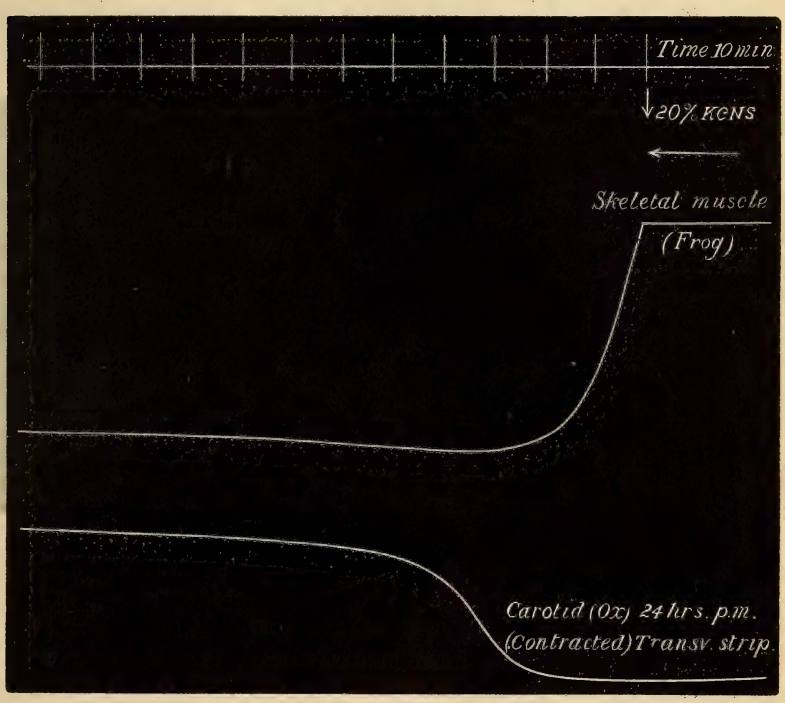


FIG. 1.

to cause complete relaxation; on the other hand, sulphocyanide causes precipitation of proteid in a saline extract of the arterial wall. In contrast with this, saline fluids like the ammonium chloride solution extract a large amount of proteid before they effect complete relaxation, and many hours are necessary for the completion of the effect (*e.g.*, 24 hours). Magnesium sulphate solutions (5 per cent.) require days.

When relaxed by sulphocyanide the artery shows a permanent loss of excitability and contractility.

Ammonia Vapour.—A contracted artery exposed to strong ammonia vapour (in the same way as described in the case of chloroform vapour) speedily begins to relax; in a very few minutes the change is well

marked, and in 10 or 15 minutes relaxation is usually complete, even in a thick-walled tube like the carotid of the ox.

Graphic records were made of the action of ammonia vapour by an arrangement similar to that used with the sulphocyanide solution. Fig. 2 shows the effect upon a transversely cut strip of the wall of a contracted artery, and upon a recently excised skeletal muscle (frog's gastrocnemius). The upward movement of both levers indicates relaxation—very extensive in the case of the artery. The ammonia vapour was applied at the moment of the first time signal.

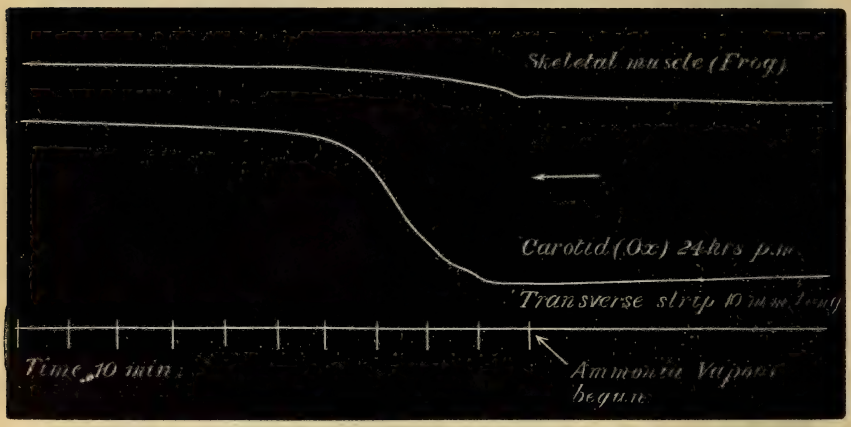


Fig. 2.

It is to be noted that when a strip of artery is suspended in this way it often contracts slightly prior to the application of any reagent —apparently a response to the stimulus of stretching or of compression of its ends by the clamps which hold it, or a combination of these causes.

Heating.—Keeping an artery at about body temperature for a number of hours (*e.g.*, 24) causes the contraction to pass off, as has already been stated.

Heating to about 50—55° C. induces complete relaxation in a few minutes. The effects of heat will be described in detail later.

Kneading, Rubbing, Stretching.—More or less extensive relaxation of a contracted artery can be speedily induced by kneading or rolling the vessel between the fingers for a minute; the wall becomes much less firm, and the lumen opens up markedly—*e.g.*, from a diameter of 1·5 mm. to twice that size or more; later it may again contract to some extent.

Mayo* observed that an artery exposed in a living animal became

* 'Outlines of Physiology,' London, 1837.

markedly relaxed when it was rubbed between the finger and thumb for a minute or two; this relaxation passed off after a time.

Somewhat similar results occur when a contracted artery is forcibly stretched by the introduction of a glass rod 5—6 mm. in diameter. Partial recovery may occur. Probably the arterial wall is injured by the very considerable amount of force required to overcome its contraction in this way.*

Effects of Heat upon Arteries and Veins and upon Saline Extracts.

I have studied the effects of heat upon the arterial wall in different ways—

(1) By direct examination and measurement of portions of excised artery, heated to various temperatures and at various rates while immersed in defibrinated blood (of the same animal) or in olive oil, or simply kept in a bottle or test-tube moistened with normal saline solution.

(2) By obtaining graphic records of the changes in length of strips of the artery, cut either transversely or longitudinally, under the influence of various temperatures. The arterial strip was placed in a bath of olive oil, or defibrinated blood (when relatively low temperatures were all that was necessary); the strip was rigidly fixed at one end, while the other was connected (by means of a fine wire) with a recording lever, suspended in the horizontal position by a long feeble spiral spring, and writing upon a slow drum; the bath was gradually heated up by a spirit lamp or Bunsen burner, usually at about the rate of one degree Centigrade per minute, though the precise rapidity of

* I have very recently found some references in John Hunter's writings, which may be quoted in connection with the subject of *post-mortem* contraction. He describes an experiment on the human umbilical cord, in which he found that when the arteries were cut across 2 days after delivery the lumen was found to be closed 24 hours afterwards; this did not occur when the section was made 3 days after delivery. The experiment was performed on a portion of the cord left attached to the placenta. Hunter also refers to the arteries being contracted after death by hæmorrhage: he speaks of the "stimulus of death," and describes the "contraction by death" as being less in the aorta than in more distant vessels ('Works,' London, 1837, vol. 3, pp. 158 and 168). In his 'Essays and Observations' (London, 1861), vol. 1, p. 133, Hunter mentions an experiment on the uterus and its vessels: "I injected the uterus of a cow that had been separated from the body of the cow about 24 hours; and I found next day that it had contracted very much, and that the vessels had also contracted, for the great trunks were more turgid than when injected, so that the injection had been squeezed back again. This also shows that the small vessels have a greater or longer power of contraction than the large ones."

In a footnote on the same page, Clift comments on the above experiment: "May not this be the effect of elasticity in consequence of the parts having been put into hot water while being injected? I have seen that happen."

heating was varied within pretty wide limits and the temperature was often kept steady at various levels for considerable periods.

In many experiments the oil bath was not heated directly by the lamp; it was immersed in an outer beaker of water, which was placed on a piece of gauze and heated in the way mentioned. In most cases two strips were heated simultaneously in the bath, and made to record on the same drum. The temperature was determined by a thermometer fixed in the oil-bath with the bulb close to the arterial strips. A time record usually showing periods of 5 minutes was simultaneously inscribed. Shortening of the arterial strips is indicated by a downward movement of its lever. The magnification was generally three times.

In many experiments the recording lever was directed at right angles to the circumference of the drum, so that its point moved in a straight line perpendicular to the direction of movement of the recording surface —not in an arc of a circle like a lever used in the ordinary way. Ordinates were drawn through the marks in the time tracing; at one end of each ordinate the exact time was inscribed, at the other end the temperature at that moment.

Instead of spiral springs small weights (*e.g.*, 1—2 grammes) were sometimes employed to maintain tension of the arterial strip during the experiment.

Slight contraction may occur before the heating has begun, and shortly after the strip has been suspended—probably due to the mechanical stimulus supplied by compression of each end of the strip by the clamp which holds it, perhaps assisted by the slight tension of the strip when connected with the recording lever.

Poisseuille* stated long ago that the force of reaction excited by distension of an artery was greater than the force used to distend it. And Bayliss† has recently described a number of experiments upon the reaction of intact arteries (isolated from the central nervous system) to the distending force of a raised blood pressure.

Frequently pieces of excised artery (unopened)‡ 2—3 cm. long were suspended in the bath alongside the arterial strips, and examined from time to time, so as to correlate the changes seen in the unopened tube with those shown graphically by the transverse or longitudinal strips. The results obtained by these two methods were entirely concordant. Experiments were made on arteries with strong muscular coats like the carotid, and also on the aorta and pulmonary artery.

Experiments on the Carotid Artery.—Heating an artery produces very

* 'Journal de Physiologie,' par M. Majendie, vol. 8, p. 272. See note in John Hunter's 'Works' (London, 1837), vol. 3, p. 157.

† 'Journal of Physiology,' vol. 26, "Proceedings of Physiological Society," p. 29.

‡ This means excised segments of artery not laid open longitudinally, though open at the ends.

different effects, according to the state of the artery at the time—whether it is contracted or relaxed; in the case of the contracted artery there are also certain differences in its behaviour at the early and the late stages of contraction.

Relaxed Artery.—An artery in a state of relaxation behaves in essentially the same way when heated, whether its relaxation is due to (*a*) the gradual passing off of *post-mortem* contraction after some days, or (*b*) to being kept at about body temperature for 24 hours, or (*c*) to its having been frozen for some hours, or (*d*) to treatment with potassium or ammonium sulphocyanide solution, or (*e*) exposure to ammonia vapour. When the temperature is gradually raised there is no important change, though slight shortening may gradually occur, until it reaches 60—65° C., when a well-marked and commonly very extensive contraction takes place; this is seen whether a transverse or a longitudinal strip is recorded, and the change is evident in an unopened segment of artery, suspended in the bath—the lumen becomes very markedly diminished, while the arterial wall becomes much firmer to the touch. When the shortening has been completed, and the temperature is allowed to fall, there slowly occurs a certain amount of lengthening, but this is always very incomplete. (Fig. 3.)

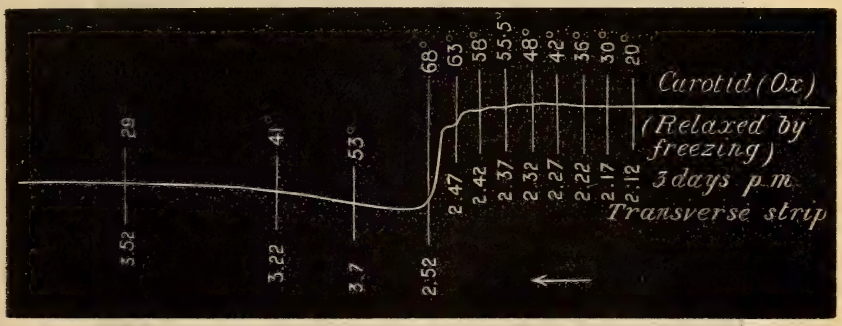


FIG. 3.—Artery (ox) relaxed by freezing. Transverse strip, 10 mm. long; load, 2 grammes. The minor oscillations preceding the extensive descent of the lever, beginning at 61°, are more distinct than usual; they are often entirely absent. Carotid (horse) relaxed by freezing. Transverse strip, 18 mm. long.

This characteristic heat-contraction at 60—65° seems to be due to the elastic and connective tissue elements of the artery, and not to depend essentially on the properties of the muscular coat. For it is well seen in pieces of artery which have had their muscle-proteids almost completely extracted by prolonged maceration in large quantities of saline fluids (ammonium chloride 13 per cent., &c.). It resembles the contraction got from a piece of tendon or of ligamentum nuchæ (ox), placed in the bath and recorded by the same apparatus.

A slender tendon (cat) was found to show extensive shortening at about 63°. Hermann* found that tendon began to contract at 65° and finished at 75°. Brodie and Richardson† describe the contraction as beginning at 60° and being complete at about 64°.

Gottschlich‡ found that a piece of ligamentum nuchæ shortened on heating and lengthened on cooling for all temperatures up to as high as 65°. Another shortening, not removed by cooling, developed between 65° and 75°.

It may be mentioned that a frog's muscle tested with the apparatus I have used for the arterial strips, shows very clearly the three heat-contractions studied by Brodie and Richardson§ with the aid of a photographic method at about 34°, at 45—50°, and at 55°—60°.

Contracted Artery.—When an artery which is in a state of *post-mortem* contraction is gradually heated, it undergoes a characteristic change at a temperature of 50—55° C.—sometimes a little below and sometimes a little above this level. It becomes relaxed, as is shown by the tracing given by a transverse strip; the rise of the lever indicates a marked elongation of the arterial strip (fig. 4); the same change is

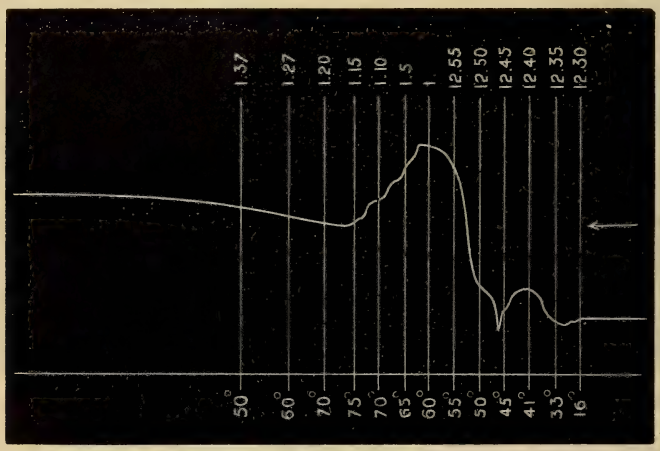


FIG. 4.—Carotid (ox). Transverse strip; 25 hours *p.m.* Load under 1 gramme.

evident in a piece of unopened artery by a notable enlargement of the lumen, while the walls of the tube lose their firmness. When this relaxation has occurred, a subsequent lowering of temperature does not restore the contracted condition. Excitability and contractility are absent after the temperature has been raised to about 50°. It is important

* 'Pflüger's Archiv' (1873), vol. 7, p. 417.
† 'Phil. Trans.,' B, vol. 191, p. 127.
‡ 'Pflüger's Archiv' (1893), vol. 54, p. 109.
§ *Loc. cit.*

to note in regard to the relaxation beginning at 50°, that the arterial strip is subjected to only a very slight amount of tension during the experiment—the spiral springs employed being long and weak; the tension may be regarded as being nearly constant throughout. Instead of springs, weights of small amount (2 grammes) were sometimes used. In the case of the unopened artery where no tension is employed, the only force (outside the muscle fibres) to cause an expansion movement is the elasticity of the arterial wall, which tends to make it assume the position which it occupies in the passive artery.

The extent of the relaxation occurring at 50—55° varies with the amount of contraction present at the time the temperature reaches that level.

When relaxation of contracted artery has begun at about 50° to be completed, perhaps by the time the gradually rising temperature has reached 58—60°, then a further rise to 60° or 60—65° leads to the appearance of the contraction already described in the case of a relaxed artery. (Figs. 6 and 7.) When the process of relaxation has not been completed at the time the changes underlying the 60—65° contraction begin, the latter is often much more slightly marked in the tracing than usual, the tendency to shortening being probably opposed and partially masked by the relaxation which is still going on. (Fig. 11.)

While relaxation at about 50° C. is the most constant and outstanding feature in the behaviour of a contracted artery when heated, there are other important features which vary according as we are dealing with (*a*) the earlier stage of *post-mortem* contraction (a few hours or a day or two after death), or (*b*) its later stage.

(*a*) During the earlier stage—while the artery is still excitable—heating commonly induces important changes long before the level of 50° is reached—often indeed pretty soon after the rise of temperature has begun. The tracing given by a transverse strip may show large curves indicating marked changes of length; there is commonly a curve before 40° is reached, and one after 40° before the extensive relaxation beginning about 50°. Contraction often begins at about 25° to reach its maximum about 35° and then relax; at 40° relaxation is usually well marked. As the temperature rises, a further contraction (sometimes very extensive) takes place, especially between 45° and 50°, and often particularly marked about 47°, to give place about 50° to relaxation. At 60—65° contraction again occurs—to relax slowly and partially when the temperature falls. (Figs. 4 and 8.) Thus when a transversely cut strip of artery from a recently killed animal is heated up to or beyond 65° and then allowed to cool, there are usually three phases of shortening and three phases of lengthening; the first shortening at 25—35°, the second at 45—50°, the third at 60—65°; the first lengthening at 35—45°, the second between 50°

and 60°, and the third during the period of cooling after the temperature of 65° has been reached. These broad features in the behaviour of the arterial strip are remarkably constant in their appearance and behaviour, though there is much variation in detail.

The extent of the first and second shortenings varies much in different arteries and at different periods after death. Commonly they show as quite large curves in the tracing, the shortening in each case being followed by lengthening. The elongation following the first shortening at 25—35° usually reaches its maximum at about 40° or a little higher, when the temperature is steadily raised. Cooling down to 35—25° does not restore the contraction seen at that phase when the temperature was being raised; evidently the 25—35° contraction is not a shortening conditioned simply by the presence of a temperature of 25—35°, but excited by a rise to that level. (Fig. 9.)

When the artery has been kept some time longer (days) before being heated, the contractions (α) at 25—35°, and (β) about 47° gradually become lessened and at a later phase disappear altogether; (β) persists longer than (α) in many arteries. (Figs. 7 and 11.) The tendency to relaxation about 40° is often seen to outlast both the contraction curves (α) and (β), though slight in amount compared to the relaxation at 50—55°, into which it often grades. (Fig. 5.)

(*b*) During the later stage (*e.g.*, several days after death) the artery—still more or less contracted—may show no very striking change till a temperature of about 50° is reached, though there is often slight relaxation beginning at about 40°. The usual 50—55° relaxation leads to a complete abolition of such contraction as is present. (Fig. 5.)

When an artery from a recently-killed animal is placed and kept in olive oil, the duration of its contraction is greatly prolonged, so that very many days after death marked relaxation occurs when a transverse strip is heated to 50—55°. As the contraction gradually diminishes after a number of days, the amount of relaxation obtained on heating lessens.

In the carotid of the horse, distinct relaxation may occur at 50—55° as long as 14 or even 18 days after death—though very much less extensive than on earlier days, when more contraction was present in the artery.

Later, when contraction has quite gone, no sign of elongation is seen at all on heating.

Fig. 5 shows the relaxation that occurs in a transverse strip of horse's cartoid kept in olive oil for 10 days *p.m.* The contraction present in the early days had become greatly diminished by that time. Fig. 6 shows that very slight but still appreciable relaxation occurred 18 days after death in the same artery; there had still been a very small amount of contraction persisting.

Tracings made 14 days and 16 days *p.m.*, showed an amount of relaxation less than is seen in fig. 5, but greater than in fig. 6.

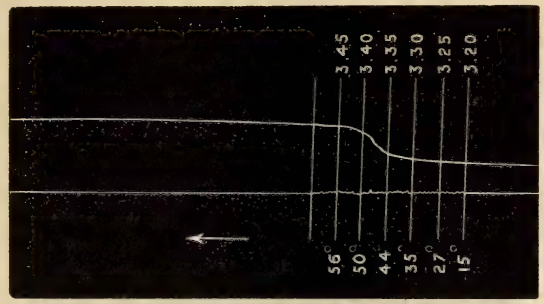


FIG. 5.—Transverse strip, carotid (horse), which has been kept in olive oil for 10 days after death.

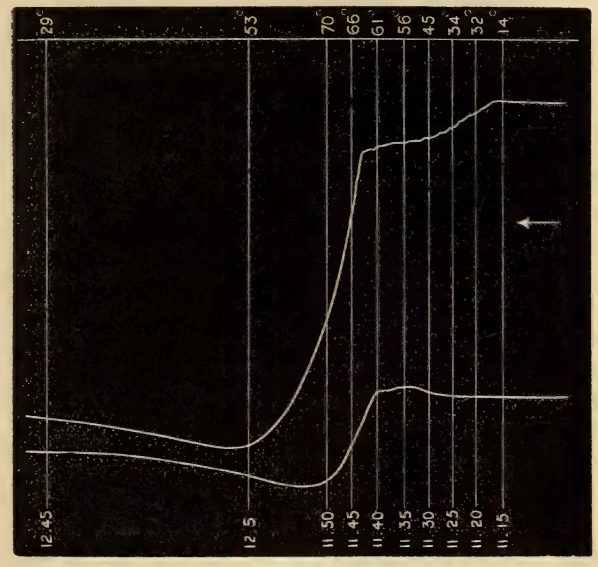


FIG. 6.—Upper tracing from transverse strip of jugular vein. Lower tracing from horse's carotid kept 18 days in olive oil.

Strips cut from a contracted artery parallel to the long axis of the vessel behave, when heated, very differently from transversely cut strips. Indeed, the former show alterations in length which are—up to a certain temperature the converse of those shown by the transverse strip, the contractions of the transverse strip between 20° and 35°, and between 40° and 50° are accompanied by elongations of the longitudinal strip—less in amount it may be, but coinciding pretty accurately in

time. Similarly the lengthening of the transverse strip about 40° and between 50° and 60° is accompanied by a certain amount of shortening of the longitudinal strip. Fig. 7 shows the relative behaviour of the two strips when heated up to about 58°.

But this converse relation ceases as the temperature rises; both strips contract markedly at 60—65°.

(3) Another method of studying the influence of heat upon the arterial wall is to use a segment of artery, one end of which is closed by tying in a wooden plug, while into the other end is fastened one extremity of a long glass tube of small bore. The interior of the artery and the glass tube for part of its length are

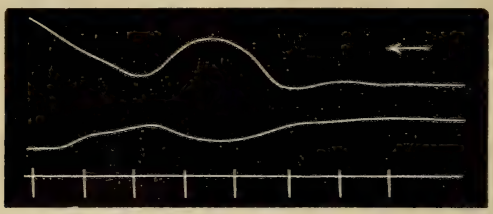


Fig. 7.—Upper tracing, transverse strip of contracted artery (carotid of ox). Lower tracing, longitudinal strip of same artery. The phases of elongation (35°—45° and 50°—58°) in the transverse strip are coincident with phases of shortening in the longitudinal strip and *vice versâ*.

filled with olive oil, and a graduated millimetre scale is placed behind the tube so that the position of the column of oil may be acurately read off. The tube is bent at right angles not far from the artery, so that the main part of the tube (with the scale) may be fixed in the horizontal position while the artery is placed vertically in an oil bath, the temperature of which is then raised in the usual way. Contraction and relaxation of the artery are indicated by the advance or retreat of the oil in the horizontal tube. The results are entirely confirmatory of those obtained with transversely cut strips from the same artery.

Effects of Temperature Changes on some other Unstriped Muscles of Mammals.

The behaviour of the contracted arterial muscle presents a notable resemblance in many respects to what is seen in some other unstriped muscles which have been examined in the living condition.

The retractor penis muscle* retains its vitality long after removal

* Eckhard, 'Beiträge zur Anat. u. Physiol.,' vol. 3, p. 123 (1863); Langley and Anderson, 'Journal of Physiology,' vol. 19, p. 85; Starling, in 'Schäfer's Text-book of Physiology,' vol. 2, p. 349 (1900); Sertoli, 'Archives Italiennes de Biologie,' vol. 3, p. 78; Schultze, 'Arch. für Physiologie,' 1896, p. 54.

from the body. Sertoli found its excitability to persist 5, 6, or even 7 days after excision—especially when kept in blood-serum at a temperature of 5—8° C. The muscle shortens very markedly on cooling and relaxes on warming to about 40°; then remains unchanged in length up to 50°; it is killed between 40 and 50°. Slow rhythmic contractions may appear when cooled from 40 to 35°, even 2 or 3 days after excision.

In a very recent paper, De Zilwa* describes the shortening of the muscle by cooling down to 10 or 15° C., gradual relaxation up to 40° (when it is complete), spontaneous contractions developing at 38° absent at 40°. On further raising the temperature he notes contraction at 47° or 48°; later, relaxation beginning at 52—54° and completed at 58—60°. The muscle is often found to retain some trace of excitability when beginning to relax at 52—54°; when the relaxation is complete the muscle is dead, and its response to excitation or changes of temperature cannot be restored by cooling. There is no evidence of the occurrence of true *rigor mortis* in this muscle.

The unstriped muscle of the cat's bladder also maintains its excitability for relatively long periods.

C. C. Stewart† finds that at ordinary room temperatures, irritability often lasts 24—48 hours after excision; kept in an ice-box at 5—8°, one preparation responded to the Faradic current at the end of 4 days. When cooled a strip of the bladder muscle shortens, the shortening being complete about 10° C. When the temperature is raised from this point it relaxes up to about 40°, above 40° there is shortening (slowly at first, then more rapidly) up to 53—57°, where the muscle apparently loses its excitability and dies. A distinct loss of tone, often of considerable extent, follows; the muscle is comparatively relaxed and very soft. Only when the temperature is raised to 69° does what Stewart describes as the shortening of heat-rigor occur.

The essential correspondence of the main features presented by these living muscles and by a contracted artery under the influence of temperature changes is too obvious to need insisting upon.

Many years ago, Samkowy‡ found that the unstriped sphincter pupillæ muscle of the rabbit contracts at first on warming, but dilates later; when relaxed by warmth, cooling to 28—29° induces contraction. The dilated pupil of a dead cat contracts at moderate temperatures and dilates when the temperature is raised to about 37°. Using also the rectococcygeus muscle of the rabbit and the bladder of the cat, rabbit, &c., he concluded that unstriped muscle relaxes at about 37°.

* 'Journal of Physiology,' vol. 27, p. 200.

† 'American Journal of Physiology,' vol. 4, p. 199 (1900).

‡ 'Pflüger's Archiv,' vol. 9, p. 400 (1874).

Morgen* heated a ring from the œsophagus of the dog and observed abolition of irritability at about 50°.

From the evidence available it is clear that relaxation at 50—55° C. is a phenomenon of widespread occurrence in living mammalian unstriped muscle showing tonic contraction; the muscle is dead when this relaxation is completed.

Experiments on the Aorta and Pulmonary Artery.

Transverse and longitudinal strips were examined soon after death in the same way as the carotid. The changes observed were similar in general character to those described in the carotid at similar periods after death—relaxation about 50—55°, preceded in many cases by contraction, &c.

But these changes are very slight in extent in the aorta and pulmonary artery, as might be expected in vessels provided with muscular tissue in relatively small amount. (Fig. 8.)

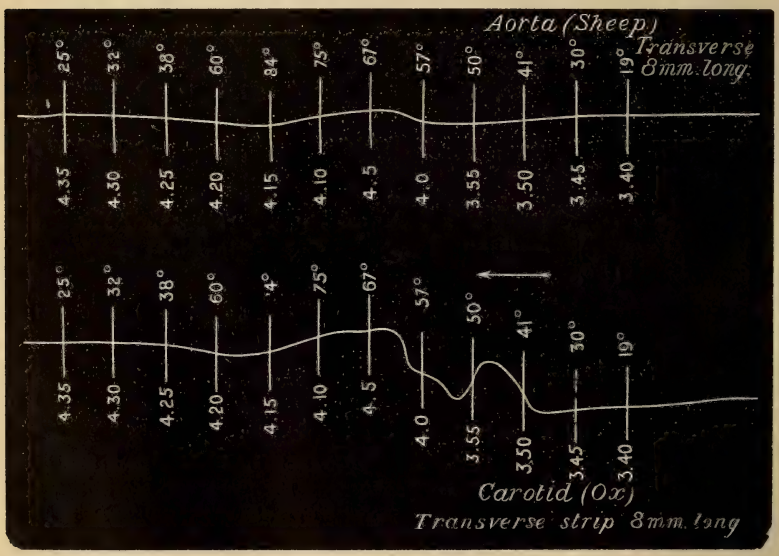


Fig. 8.

At the same time the contraction at 60—65° is hardly so extensive as might have been looked for in strips containing so much elastic and connective tissue.

* 'Untersuch. aus d. Physiol. Inst. zu Halle,' vol. 2, p. 161 (1890).

Experiments on the Jugular Vein.

Strips of vein when heated gave very definite and constant results. Shortening began almost immediately, and went on at first pretty quickly (from the original temperature of 14—16° up till about 30°), then more gradually, till at 60—65° (usually at 62° or 63°), rapid and very extensive shortening took place, going on commonly till the temperature is above 70°. Subsequent cooling is attended by a decided elongation, though the strip still remains very much shorter than it was to begin with. (Figs. 6, 9, and 10.)

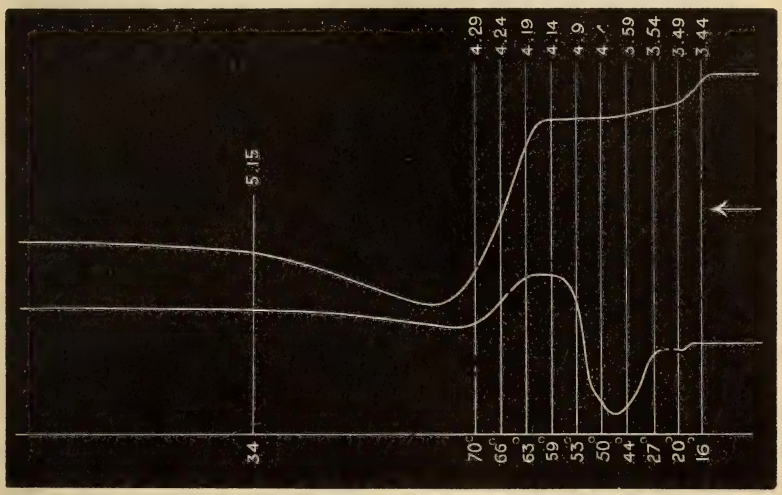


FIG. 9.—Upper tracing from transverse strip of jugular vein, lower from carotid artery of horse.

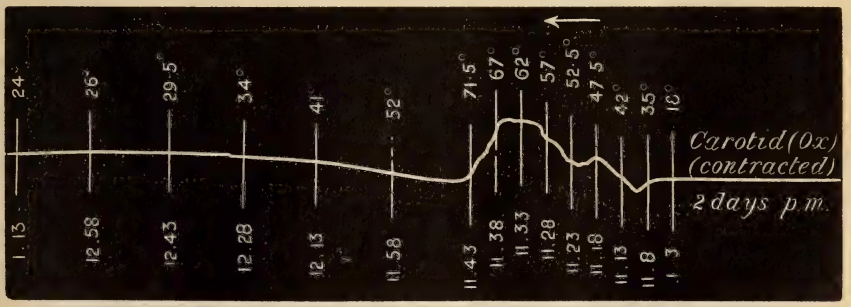


FIG. 10.

When the temperature is only raised to 40° and then allowed to fall, shortening goes on as before up to that point, and the strip

lengthens again during cooling, so that it returns to the same, or almost the same, length as it was at first. (Fig. 11.)

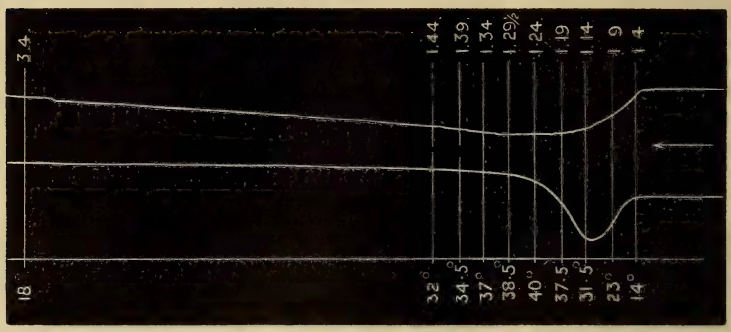


FIG. 11.—Upper tracing, jugular vein; lower, carotid. Transverse strips, heated up to 40° and then allowed to cool.

Saline Extracts of Contracted Artery.

Saline extracts of the fresh arterial wall were made from the carotid, &c., usually with 5 per cent. $MgSO_4$ solution; 10 per cent. NaCl and 13 per cent. NH_4Cl were occasionally employed. The outer and inner coats were often stripped off as completely as possible, so that little more than the tunica media was used.

The 5 per cent. $MgSO_4$ extract is usually neutral.

It was heated up gradually by Halliburton's method, sometimes without acidulation, sometimes faintly acidulated with 2 per cent. acetic acid. The rise of temperature was commonly about the rate of 1° C. per minute, though this was made to vary widely, and the fluid was often kept at certain temperatures for many minutes.

Acidulated Extract.

Coagulation occurred between 45° and 50°, usually about 47°. When this was filtered off and the heating continued, a second coagulation took place at 55—60°—figures corresponding with the coagulation temperatures of Halliburton's paramyosinogen and myosinogen.

(There was some evidence of a further slight coagulation at about 72—75°.) The amount of coagulation occurring at 45—50° and at 55—60° varied very markedly; sometimes the one and sometimes the other predominated. It is to be noted that when the preliminary acidulation is done a certain amount of precipitation occurs—probably nucleo-proteid, in part at least. After filtration, the liquid is treated as above described.

Neutral Extract.

The fluid became strongly opalescent at 45—50°, but as a rule did not become flocculent, and no precipitate could be separated by filtration. (Compare Vincent and Lewis's results with non-striped muscle from the stomach of the calf; these authors remark a similar absence of definite coagulation in neutral extracts.*)

Well-marked coagulation became evident at 60—65° as a rule.

Saturation with $MgSO_4$ appeared to precipitate nearly all the proteid contained in the saline extract. Shaking was not employed in effecting saturation; the extract was allowed to stand for 24 hours with excess of $MgSO_4$ crystals, and inverted from time to time.

Small test-tubes, containing 5 per cent. $MgSO_4$ extract of the arterial wall (neutral or acidulated), were often placed in the oil-bath, in which strips of the same artery were being heated and made to record their changes in length; the changes in the strips and the $MgSO_4$ extract placed side by side under the same conditions were compared.

Contracted Artery.

The early contraction frequently occurring (25—35°) in the strip was attended by no evident change in the $MgSO_4$ extract, nor was the relaxation about 40°. The contraction frequently seen between 45° and 50°, especially about 47°, coincides with the development of marked opalescence in the neutral $MgSO_4$ extract, and with coagulation in the acidulated extract.

The characteristic relaxation at 50—55° follows upon the change in the $MgSO_4$ extract just mentioned, and corresponds with no definite further change in that liquid.

The final shortening of the strip at about 65° takes place pretty much at the same time as coagulation in the extract, but evidently does not essentially depend on that coagulation, since it occurs in strips that have been macerated for many days in large quantities of 13 per cent. NH_4Cl solutions.

When an acidulated $MgSO_4$ extract is used to compare with the changes occurring in the heated strip, the preliminary precipitation caused by the addition of a little 2 per cent. acetic acid has to be borne in mind. For the substance thus precipitated and removed by filtration might possibly have been one which would have shown a change in relation to some of the phases in the behaviour of the arterial strip during heating.

Of course this objection does not apply when a non-acidulated $MgSO_4$ extract is used in making the comparison.

An attempt was made to co-ordinate the changes occurring in a

* 'Journal of Physiology,' vol. 26, p. 445.

$MgSO_4$ or NH_4Cl extract when heated with those occurring within the tissues of the arterial wall in the following way:—Weighed amounts of the arterial wall were taken and heated at the usual rate up to various temperatures (40°, 45°, 50°, 55°, 60°, &c.), immersed in mercury contained in small (corked) test-tubes, which were placed in the oil-bath. The portions of artery so treated were then extracted with equal amounts of 5 per cent. $MgSO_4$ or 13 per cent. NH_4Cl solution for equal periods, for comparison with portions of the same artery which had not been heated at all. The different extracts were tested for the amount of proteid which they contained by heat-coagulation, precipitation by various reagents, &c.

It was found that heating fresh artery up to 40° or 45° made no appreciable difference in the amount of proteid subsequently obtainable from it by 5 per cent. $MgSO_4$ sol., while heating to 49° or 50° markedly diminished the subsequent yield of proteid. Obviously a considerable amount of proteid has been rendered insoluble at 45—50° in the tissue. Heating to 55° did not seem to make any marked difference as compared with heating to 50°. A temperature of 60° caused the subsequent yield of proteid to be greatly lessened; indeed there was little extracted by the $MgSO_4$ sol. after the artery had been heated to 60°. Evidently coagulation had taken place between 55° and 60°.

Comparing these results with what is seen on heating the acidulated $MgSO_4$ extract of an artery that has not been heated, we find that proteid is rendered insoluble in the tissue of the arterial wall at about the same temperatures as in the extract. In the case of the non-acidulated extract, proteid becomes insoluble in the tissue (45—50°) when the extract becomes strongly opalescent, but shows no floccules capable of separation by filtration; later (55—60°) proteid is rendered insoluble at a somewhat lower temperature than is usually required to give a flocculent heat-coagulum (60—65°) in the non-acidulated extract.

Changes in the length of strips of the arterial wall bear a relation to the coagulation of proteid in the tissue similar to what has already been stated in regard to $MgSO_4$ extracts, excepting that the rendering insoluble of proteid at 55—60° in the tissue comes distinctly before the final shortening of the strip.

Saline Extracts of Relaxed Artery.

Some of the methods already described as effective in relaxing a contracted artery would obviously render the subsequent making of saline extracts impracticable (sulphocyanide, &c.).

The freezing method is not open to this objection. Extracts made with 5 per cent. $MgSO_4$ from an artery relaxed in this way show heat

coagulation (when acidulated) at 45—50° and 55—60°; also arteries that have been kept for days until contraction has passed off, and arteries relaxed by keeping at body temperature for 24 hours, &c.

Now strips from such arteries—if completely relaxed—show no important change when heated until the 60—65° shortening takes place; there is no alteration in the tracing coincident with the heat-coagulations at 45—50° and at 55—60°, which are seen in the $MgSO_4$ extract.

Saline Extracts of Aorta and Pulmonary Artery.

These when acidulated and heated show relatively little proteid coagulation, as might be expected from the relatively small amount of muscle in their walls; the temperatures of coagulation seem to be pretty similar to those of extracts of carotid.

Contraction v. *Rigor Mortis.*

Is the *post-mortem* contraction seen in an artery a true contraction of exceedingly long duration, or is it a condition of the same nature as the rigor mortis of skeletal muscle?

There is a variety of evidence in favour of the conclusion that *post-mortem* contraction of arteries is a true persistent contraction, very different in many respects from the rigor mortis of skeletal muscle.

(1) As has been already stated, though the excised artery goes into the contracted state very soon after death, its excitability may be maintained for two or three days. Up to this time then it is obvious that the arterial contraction has been of a nature very different from true rigor mortis. The same is indicated by the partial relaxation of the contracted artery, which may spontaneously develop a day or two after death—to be followed by further contraction of the application of a suitable stimulus. The reaction of the cut surface to litmus paper is alkaline or amphoteric, even after the contraction has lasted for days.

(2) The effect of sulphocyanide of potassium solution applied to a contracted artery is strikingly different from what is seen in the case of a skeletal muscle contracted in rigor mortis. The artery, as has already been described, soon becomes completely relaxed in the solution, whereas the rigor mortis muscle does not relax at all.

(3) The influence of freezing for some hours is very different in the two cases. In the artery freezing quickly after the death of the animal, before *post-mortem* contraction has begun, entirely prevents the appearance of that contraction; in skeletal muscle similar freezing does not obviate the subsequent development of rigor mortis (with acid reaction, &c.). Portions of skeletal muscle from the ox, horse, &c., were frozen for four hours, and showed a marked contrast to arteries

similarly treated; the skeletal muscle went strongly into rigor mortis after thawing.

Again, it has been shown that freezing a contracted artery for some time (*e.g.*, 4—5 hours) completely and permanently abolishes the contraction. In rigid skeletal muscle the result is different; in some cases there is no perceptible diminution in the rigidity after thawing; in other instances, the rigidity seems to be diminished but by no means abolished.

(4) The effect of heating a contracted artery is strikingly different from what occurs when a skeletal muscle is treated in the same manner.

The contracted artery shows a characteristic relaxation at about 50° C., which may or may not be preceded by definite phases of contraction and relaxation, as already described; skeletal muscle in rigor mortis shows no such relaxation. In its behaviour towards changes in temperature the contracted artery shows an unmistakable resemblance to what is seen in non-striped muscle that is unquestionably alive, *e.g.*, the muscle of the cat's bladder and the retractor penis already referred to; there is an essential agreement in the main features evident in each case—increased tonus on cooling, relaxation at about 40°, followed by shortening, relaxation at 50—55° with final abolition of tonus, &c.

(5) When a strip of contracted artery is stretched by the successive addition of equal increments of weight, its behaviour (as will be presently described) is entirely different from that of a rigid skeletal muscle. A strip of relaxed artery, on the other hand, gives results essentially similar in their general character to those yielded by skeletal muscle.

(6) When a contracted artery is distended by internal pressure, it can often completely recover its original volume in the contracted state when the distending force is removed; indeed, in some instances it shows a subsequent increase of the original contraction. This is in sharp contrast to rigid skeletal muscle which, as is well known, fails to return to its former length when it has been stretched by the application of weights.

Elasticity of Strips of the Arterial and Venous Walls.

Wertheim,* and all observers who have worked at the subject since his time, have found that when strips of the arterial wall (aorta commonly used) are stretched by the successive addition of equal increments of weight the amounts of extension produced do not remain constant, but go on diminishing; the coefficient of elasticity increases with increased stretching.

* 'Annales de Chimie et de Phys.,' 3e série, vol. 21, pp. 385—414 (1847).

None of these workers seem to have taken into account the possible conditions of contraction and relaxation, or to have been aware of the existence of *post-mortem* contraction at all.

The method I have employed is an old one with certain modifications. The strip is firmly held by a clamp at one end, and suspended in the vertical position; the other end of the strip is made to pull upon the lever of a Helmholtz myograph, the attachment being made relatively near the fulcrum—the extension of the strip is magnified six times by the movement of the writing point. The lever is directed at right angles to the circumference of the smoked drum and its point, therefore, moves in a straight line perpendicular to the direction of movement of the recording surface. A simple arrangement on Pflüger's plan —somewhat similar to that figured by C. C. Stewart*—is employed to keep the writing point in constant contact with the smoked paper. The successive elongations are represented by vertical lines upon a stationary drum, which was each time moved round a certain distance by hand.

Short strips (commonly 5 × 5 mm.) of the arterial wall were used in order that the angular movement of the lever should not exceed a small limit. The successive weights were allowed to pull upon the strip for equal periods—varying in actual duration in different experiments—most commonly for 1 minute. Increments of 20 grammes were used in all the experiments of which tracings are given, except where otherwise specified. Drying of the strips was prevented by frequent pencilling with defibrinated blood, serum, or salt solution (0·75 per cent.).

Very different results were obtained with (1) strips taken from large arteries like the aorta and pulmonary artery, with their highly elastic walls, and relatively scanty muscular tissue; and (2) strips taken from medium-sized arteries like the carotid, &c., with their strong muscular coat.

Transverse and Longitudinal Strips from the Aorta and Pulmonary Artery.—Both longitudinal and transverse strips were taken from the walls of these vessels (chiefly from the sheep), and the results obtained quite agree in a general sense with those described by Wertheim, Roy, and others. The greatest amount of extension is produced by the first addition of weight, and successive additions of weight cause diminishing increments in length per unit increase of weight. A line joining the bases of the extensions is a curved line concave to the axis or abscissa. (Figs. 12—15.)

Strips from Medium-sized Arteries (*Carotid, &c.*).—Strips cut longitudinally and transversely to the long axis of the artery were employed, and certain differences were evident in the behaviour of the two kinds. But much more striking and important differences are shown by strips obtained from contracted arteries as compared

* 'American Journal of Physiology,' vol. 4, p. 186.

with those taken from relaxed arteries; transverse strips exhibit the most notable contrasts. As the condition of *post-mortem* contraction does not seem to have been taken into account by previous observers or known to them, it was obviously important to test the arterial wall in the conditions of contraction and relaxation respectively.

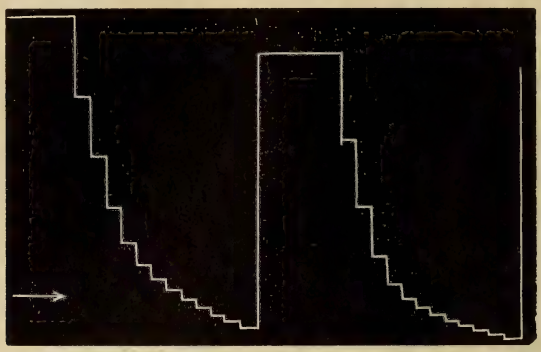


FIG. 12.—Aorta (sheep). Transverse strip. Interval of 12 minutes after unloading; then loaded a second time after the drum had been moved on about 1 cm.

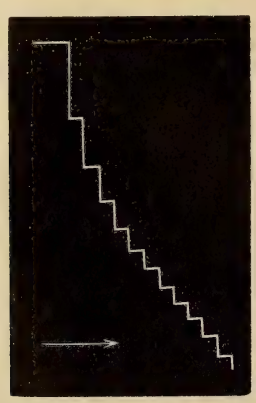


FIG. 13.—Aorta (sheep). Longitudinal strip.

Relaxed Arteries.—Both transverse and longitudinal strips made from a relaxed artery show broad features that are similar in the main; the artery may be relaxed by any of the methods already described.

The result obtained is similar to what has just been described in the case of the aorta and pulmonary artery.

Effects of repeated Stretching.—I have made numerous experiments on the effect of again stretching a strip of arterial wall some little time after it has been stretched by the addition of weights in the usual way and then unloaded.

The general character of the tracing is not altered in the aorta and pulmonary artery or in the relaxed carotid, though the actual amount of stretching is much increased, especially with the first weight or two. (Figs. 12, 14, 15, and 16.)

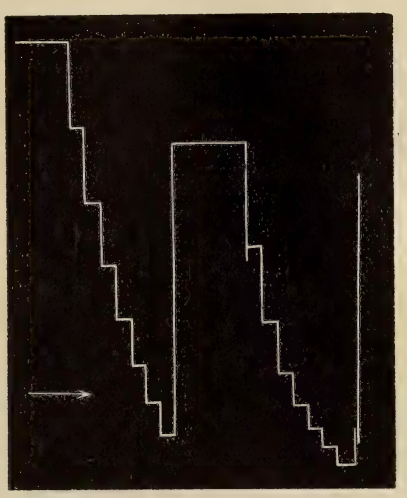


FIG. 14.—Pulmonary artery (sheep). Transverse strip. Loaded a second time after an interval of 7 minutes.

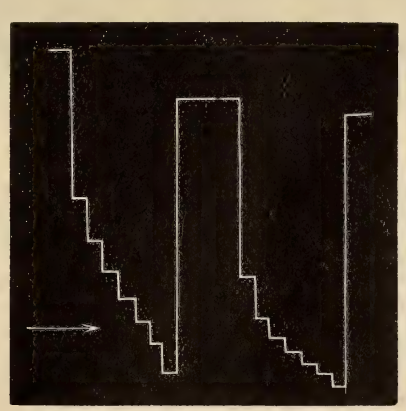


FIG. 15.—Pulmonary artery (sheep). Logitudinal strip. Loaded again after an interval of 7 minutes.

Contracted Arteries.—Transversely cut strips of contracted artery yield remarkable results, entirely different from those just described. Equal increments of weight cause increments of elongation, relatively small at first, and increasing in magnitude up to a maximum, beyond

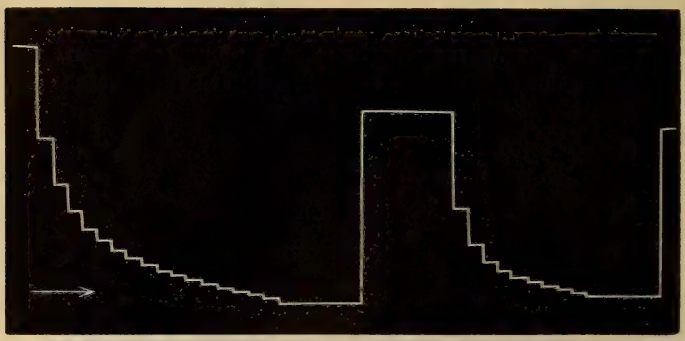


FIG. 16.—Carotid (ox), relaxed. Transverse strip. Loaded again after an interval of 12 minutes.

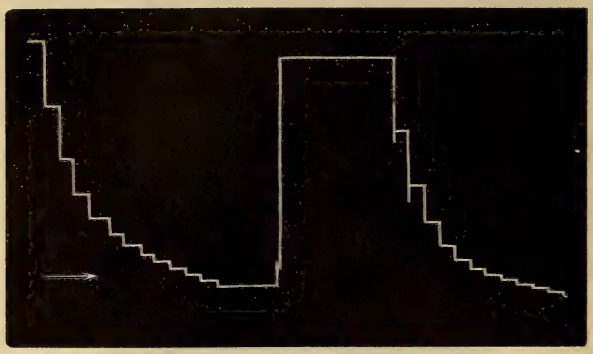


FIG. 17.—Carotid (ox), relaxed. Longitudinal strip. Loaded a second time after an interval of 10 minutes.

which there comes a progressive diminution in the amount of elongation produced by each successive addition of weight. Hence a line joining the lower ends of the vertical lines representing extension is a curved line at first convex towards the axis and then concave. (Fig. 18.)

In the early part of the process of stretching a contracted artery the resistance is solely muscular; later, when the muscular resistance has been so far overcome that the strip is stretched to what would be its normal length in a passive or relaxed artery, further stretching brings into play the resistance of the elastic and other elements in the arterial walls, and, as we have seen, a strip from such an artery resists elongation more and more, with successive increments of stretching force.

The behaviour of a transverse strip from a contracted artery is so constant and characteristic that it is always easy to tell from the tracing obtained whether the artery was contracted or relaxed. I am

not aware that any other tissue or structure has been shown to give a tracing of this sort.

These results are strikingly different from those of Wertheim,* Roy,† and others; these workers employed (chiefly or exclusively) strips of aorta in which little muscular tissue is present, frequently taken from the human subject some considerable time after death (from various diseases). Their tracings show a diminishing amount of elongation for unit weight, the same result that I have obtained in

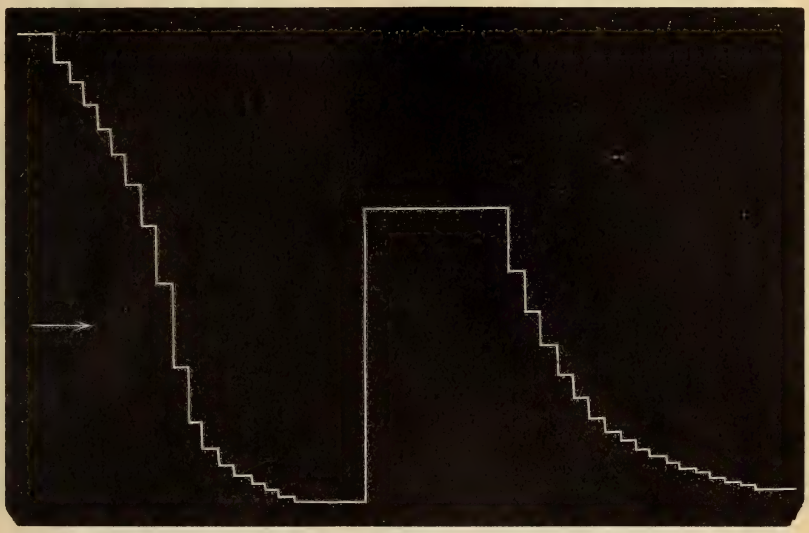


FIG. 18.—Carotid (ox), contracted. Transverse strip; 8 hours *p.m.* Second loading after interval of $1\frac{3}{4}$ hour.

the aorta and pulmonary artery. Some of Roy's curves are at first nearly, though not quite, straight in the early part of their course, *i.e.*, the elongation was during that part nearly, though not quite, proportional to the stretching weight; then a more marked and progressive diminution in extensibility becomes evident.

In some of my tracings the increment of length caused by the application of the first weight is very small—the smallest of an increasing series; in other cases the first extension is more considerable, and is followed by a very small one, to which others succeed in progressively increasing series. As regards the extent of the first elongation much seems to depend on the exact arrangement of the strip before the first weight is applied, and on the degree of its curvature, &c.

In some instances when a strip of artery taken not very long after

* *Loc. cit.*

† 'Journal of Physiology,' vol. 3, p. 125.

death is used, the series of increasing extensions is broken by a number of smaller extensions to give place again to larger increments. This is probably due to a contractile reaction of the muscular tissue of the strip against the stretching force, or is a result of mechanical stimulation caused by the clamps grasping each end of the strip. (Fig. 19.)

Repeated Stretching.—When a transverse strip is weighted in the way described and then unloaded, and after a time again weighted in the same way, a second tracing is obtained differing very strikingly from the first. It is in fact essentially similar to that obtained from a strip of relaxed artery. The extension caused by the first weight is very great. A third experiment of the same sort gives results essentially similar to those of the second experiment. (Figs. 18 and 19.)

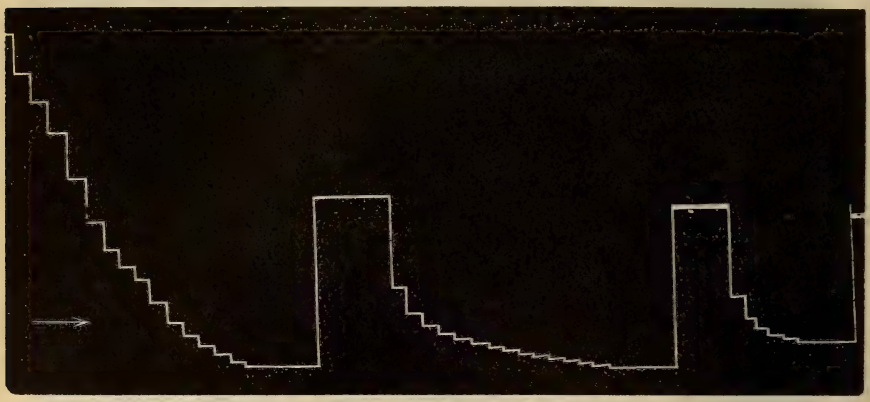


FIG. 19.—Carotid (ox), contracted. Transverse strip. During the first loading the largest extensions were followed by two or three small ones; these were succeeded by decidedly larger ones which gradually diminished in size.

Longitudinal Strip of Contracted Artery.—With strips of similar dimensions cut longitudinally from the arterial wall, the total elongation produced by a total load similar to that used with transverse strips (*e.g.*, 240—340 grammes) is very much less than with transverse strips —commonly between a half and a third.

The character of the tracing differs in the two cases. In the longitudinal strip there is only a slight increase, if any, in the amount of elongation produced as the earlier weights are added, then a progressive diminution.

After unloading, the longitudinal strip recovers much more readily and completely than the transverse strip; the former shortens quite to its original length. (Fig. 20.)

Further, a repetition of the process of loading does not cause the striking alteration in the tracing seen with the transverse strip; with

the longitudinal strip the second loading gives a result essentially similar to the first.

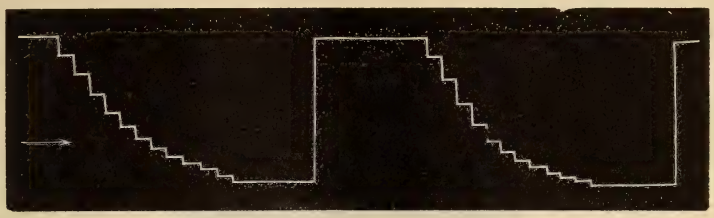


FIG. 20.—Carotid (ox), contracted. Longitudinal strip. Second loading after interval of 12 minutes.

Jugular Vein.—Transverse* and longitudinal strips give results very similar to relaxed arteries, though of course much smaller weights are required to stretch them; increments of 5 grammes were used instead of 20 grammes.

When stretched a second time, the difference in elongation is much slighter than in arteries; a longitudinal strip of vein may indeed show no appreciable difference. (Figs. 21 and 22.)

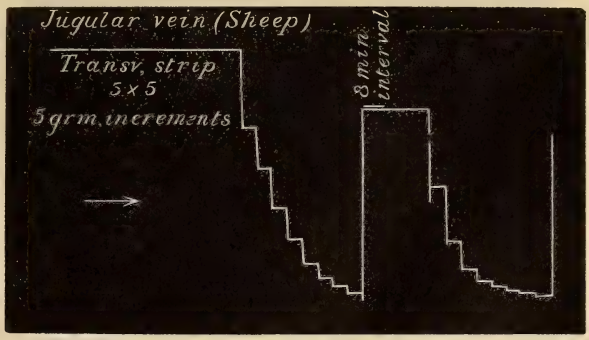


FIG. 21.

Contracted Artery treated with Salicyl-sulphonic Acid.—A contracted artery which was left in 2 per cent. watery solution of salicyl-sulphonic acid for 24 hours with the result that its proteid constituents were precipitated, gave a series of elongations almost proportional to the weights employed, recalling the results got with iron wire or india-rubber. This is well seen in fig. 23.

A second application of the weights—if the first stretching was not

* *Cf.* Braune's 'Beiträge z. Anat. u. Phys.,' 1874 p. 7, and Bardeleben's 'Jenaische Zeitschrift,' vol. 12, p. 40, 1878, on the changes in length of veins when weighted.

extensive—may give a tracing differing relatively little from the first loading—in marked contrast with what happens in a contracted artery. The elongation caused by the first weight (in the second loading) is

Fig. 22.

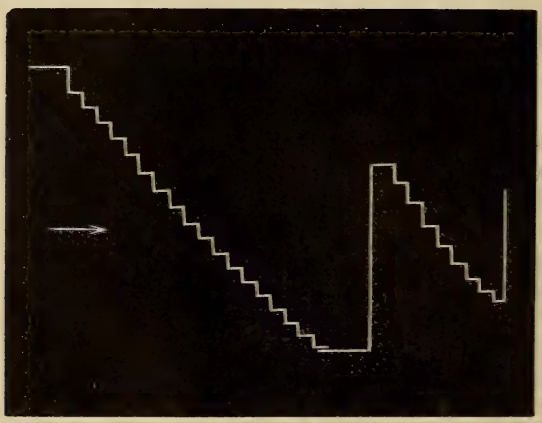


Fig. 23.—Carotid (ox), in contracted state, a few hours after death was put into 2 per cent. solution of salicyl-sulphonic acid and kept in the solution for 24 hours. Transverse strip then loaded. Second loading after interval of 15 minutes.

small; the subsequent ones are somewhat larger than before—to diminish after a time. When the first stretching is very extensive, the second weighting may give results more resembling those got with a contracted artery.

Relation of Cubic Capacity of an Artery to Internal Pressure.

Various observers have investigated this subject, but their results are very discordant.

Marey,* working with the aorta of man and different animals, and using a water-plethysmograph, found that the expansion (resembling the extension of strips of the vascular wall which Wertheim† had first described), follows the rule that the higher the absolute pressure the less the artery expands with equal rises of pressure.

Roy‡ found that in healthy arteries taken immediately after death the increase of capacity with unit increase of pressure became progressively augmented up to a certain point, beyond which the increase of capacity declined. The turning point he found to be about the normal level of blood pressure in the animal from which the artery was taken; he formulated the conclusion that the arteries are most elastic and distensible at pressures corresponding more or less exactly to the normal blood pressures to which they were exposed during life (dog, cat, rabbit, &c.).

In cases where there had been marked marasmus before death, Roy found the arteries more distensible than normal arteries; they expanded most readily at lower pressures, and he sometimes found the maximum distensibility to be immediately above zero pressure—as he found to be normally the case with veins.

Zwaardemaker§ found the augmentation of cubic capacity to be at its maximum in the excised arteries of the horse and dog at 32—50 mm. Hg. (very different figures from Roy's), in the ox at 100—150 mm. In an experiment on a living artery *in situ* (dog) he concluded that the maximum distensibility was at 75—100 mm.‖

Thoma and Kaefer¶ examined the increase of the diameter of certain arteries (external iliac and common carotid of man). They found that as the pressure was elevated the diameter increased rapidly at first, then more and more slowly.

Tigerstedt** calls attention to the discordant nature of these results, and the need of further investigation.

The relation of the cubic capacity of an artery to internal pressure I have studied by the following method:—

A portion of the artery to be examined was closed at one end by a wooden plug firmly tied in; into the other end a cannula of suitable size was made fast, through which the artery could be subjected to any

* 'Travaux de Laboratoire,' vol. 4, p. 178 (1880).

† *Loc. cit.*

‡ *Loc. cit.*

§ 'Nederlandsch. Tijdschrift voor Geneeskunde,' 2 Reeks, vol. 24, 1, pp. 61—76 (1888).

‖ Quoted in Tigerstedt's 'Lehrbuch des Kreislaufes' (1893), p. 319.

¶ 'Arch. f. Path. Anatomie,' vol. 116, p. 9 (1889). *Cf.* Luck, 'Ueber Elasticitätsverhältnisse gesunder u. kranken Arterienwand,' Inaug. Dissert., Dorpat, 1889; Kaefer, 'Zur Methodik der Elasticitätsmessungen au der Gefässwand,' Inaug. Dissert., Dorpat, 1891.

** *Loc. cit.*

desired pressure of air by means of a movable mercury reservoir communicating with a pressure bottle, from which in turn a tube leads to the arterial cannula; this tube has a lateral connection with a mercury manometer to show the amount of pressure acting upon the internal surface of the artery. The artery thus prepared is inclosed in a sort of plethysmograph consisting of a glass vessel filled with olive oil, and provided with a long graduated tube of small bore projecting horizontally at one side; the size of the bore employed varied in different experiments—commonly 1 mm. for contracted arteries, and 3 mm. in experiments on relaxed arteries (ox). Variations in the volume of the artery were accurately indicated by the movement of oil in the horizontal tube. In the figures the rises of pressure are taken as abscissæ and the expansions in volume (indicated by the number of millimetres the oil moves in the graduated tube) as ordinates. The length of the portion of artery used was measured between the ligatures fastening the cannula and the wooden plug *in situ* at either end of the artery.

The pressure was usually raised by increments of 20 mm. Hg, and kept steady at each level for 1 minute.

As might be anticipated, great differences were observable in the behaviour of relaxed and of contracted arteries.

Relaxed Artery.—Completely relaxed arteries when subjected to equal increments of internal pressure do not respond by equal increments of cubic capacity; the increase in volume is greatest at first and successively diminishes as the pressure is raised—the distensibility of the vessel is greatest at the first rise above zero. This result is similar to what has already been described as holding good in the case of veins; it is in entire opposition to what Roy found in healthy arteries examined soon after death. The expansion of volume caused in the relaxed artery by each successive elevation of pressure goes on pretty quickly at first each time the pressure is raised, then proceeds much more slowly during the latter portion of the period during which the pressure is kept steady at any particular level. (Figs. 24 and 26.)

When the pressure is raised a second time after an interval, the distensibility of the artery is found to be greatly increased. (Fig. 25.)

I have tested the elasticity of arteries (chiefly of ox and sheep) in which relaxation has been induced in the different ways already described. No doubt these various modes of treatment induce changes in the arterial wall very different in their nature in some respects. But they have one feature in common, inasmuch as they all abolish contraction, and concurrently they lead to a characteristic change in the behaviour of the tube towards internal pressure, in virtue of which the artery becomes most distensible at low pressures—immediately above zero pressure, &c.

While an idea of the behaviour of arteries relaxed by keeping and by

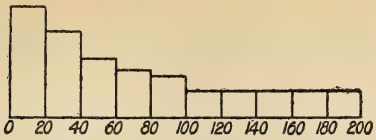


FIG. 24.—Carotid (ox), relaxed, having been kept in defibrinated blood for several days. Length (between ligatures) 14 mm. Diameter of lumen 5 mm.

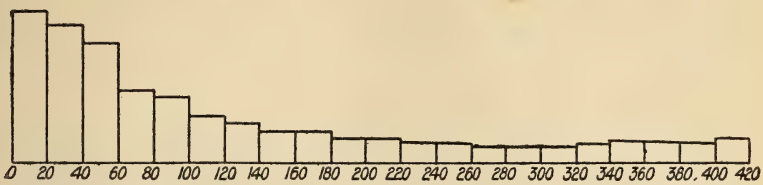


FIG. 25.—Second elevation of pressure in same portion of artery as in fig. 24, after an interval of 30 minutes. Great increase in distensibility, with comparatively low pressures. After the expansion has declined very greatly (pressure of 300 mm., &c.) there is again a slight increase of expansion between 320 mm. and 420 mm.

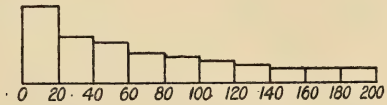


FIG. 26.—Carotid (ox), relaxed by freezing for 5—6 hours. Lumen 5—6 mm. Length of artery 19 mm.

freezing is given by figs. 24 and 26, the nature of the results obtained with arteries relaxed by other methods will be seen from the following instances. The amount of expansion is indicated by the movement of oil in the graduated tube stated in millimetres :—

Carotid (Ox) relaxed by Potassium Sulphocyanide.—A contracted artery 37 mm. long, with lumen of 3 mm., and thickness of wall nearly 2 mm., was put into 20 per cent. solution of potassium sulphocyanide for an hour or two. It was then found to be 34 mm. in length, with lumen about 5 mm., and thickness of wall 1·5—1·75 mm. Rigidity gone. Length of portion used (measured between ligatures) 17 mm. Pressure raised in usual way.

Pressure.—	0	20	40	60	80	100	120	140	160	180
		200	220	240	260	280	300	320	340	360
		380	400	420.						
Expansion.—		14	13	9	8	6	6	5·5	4·5	4·5
		4·5	5	5	55	5	5	5	4·5	4·5
		4·5	4·5.							

Pressure was then lowered to 0. An hour later the artery had not yet diminished to its original volume—the oil was 28 mm. from the starting-point.

Pressure again raised.

Pressure.—	0	20	40	60	80	100	120	140	160	180	200.
Expansion.—		18	14	12	10	6·5	7	4·5	4·5	3·5	4·5.

Carotid (Ox) relaxed by keeping it in blood at 39° *C. for* 24 *hours.* Lumen 6 mm. Length of artery used 16 mm.

Pressure raised as before.

Pressure.—	0	20	40	60	80	100	120	140	160
		180	200	220	240	260	280	300.	
Expansion.—		14	11	10	10	10	10	9	8
		10	10	8	9	8	6	6.	

Pressure lowered to 0.

An hour and three-quarters later oil was 20 mm. from starting-point.

Pressure again raised.

Pressure.—	0	20	40	60	80	100	120	140	160	180	200.
Expansion.—		27	21	16	11	7	6	6	5	3	4.

Carotid (Ox) relaxed by Ammonia Vapour.—A strongly-contracted artery exposed to ammonia vapour changed as follows:—Length diminished from 28 to 24 mm.; lumen enlarged from 2 mm. to 5—6 mm.; wall thinned from 2 mm. to about 1 mm. Length of artery used in plethysmograph, 13 mm.

Pressure.—	0	20	40	60	80	100	120	140	160.
Expansion.—		12	9	6	5	5	5	4	4.

Contracted Artery.—When a portion of artery is excised from a recently killed animal and prepared for experiment upon its distensibility in the way described above, it is usually in a state of pretty strong contraction, the contraction present on excision being as a rule markedly augmented by the necessary manipulation, tying in the cannula and wooden plug, &c. When the internal pressure is raised step by step by equal increments (*e.g.*, of 20 mm. Hg.), the artery at first yields relatively little and the augmentation of its volume is slight. Successive rises of pressure cause increasing amounts of expansion, and in the case of a thick-walled and strongly-contracted artery (*e.g.*, carotid of ox) the increasing expansion may go on up to very high

pressures (*e.g.*, 420 mm.), the muscular resistance being more and more overborne by the successive increments of internal pressure. The expansion caused by each rise of pressure is very gradual—in marked contrast to what is seen in a fully-relaxed artery; the oil in the horizontal graduated tube keeps progressing along the tube during the period of raised pressure. The increase in capacity with unit increase of internal pressure may go on increasing up to pressures vastly higher than the normal blood-pressure of the animal. (Fig. 27.) Beyond a

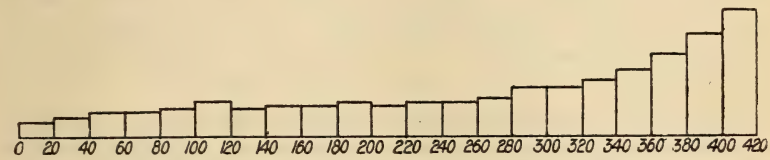


FIG. 27.—Carotid (ox), strongly contracted (48 hours *p.m.*).

certain pressure, varying in different cases, diminished expansion occurs, though in a strongly-contracted artery an enormous pressure is required to reach this point—at least when the pressure is raised at the rate adopted in the experiments now being described.

When an artery is taken which presents a slighter muscular resistance to distension—on account of its muscular coat being relatively thin, or its contraction being less strongly developed, the increase of capacity with unit increase of internal pressure becomes progessively augmented up to a certain point—which is more easily reached—and then declines. (Fig. 28.) The turning-point—indicating the maximum

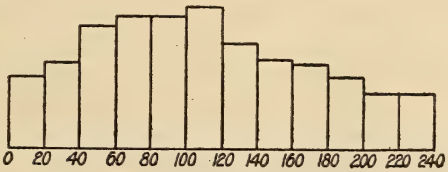


FIG. 28.—Carotid (ox), weak contraction (5 days *p.m.*). Length 12 mm.

distensibility of the tube—may come at various levels of pressure according to circumstances; it may or may not correspond to the normal height of the blood pressure in the animal from which the artery was taken. The change seems to take place some time after the muscular resistance is so far overcome, that the artery has become distended to its normal size in the passive condition; the elastic resistance of the arterial wall (now becoming stretched beyond its size in the passive state) comes into play to resist further distension. In the process of distension, when the arterial wall is being moved from the position which it occupies in a contracted artery to its position in

a passive condition, the resistance must evidently be entirely muscular, since the purely elastic property of the arterial wall tends to make it take up the position seen in a passive artery. Increased opposition to further distension will be offered by the elasticity of the arterial wall resisting distension beyond its position in the passive (relaxed) artery.

In arteries with relatively slight contraction present, the maximum distensibility is found to occur at a relatively low level—which may be about the normal blood pressure or much below the normal blood pressure of the animal. (Fig. 28.) When portions of the same artery are tested on successive days when *post-mortem* contraction is diminishing and passing off, it is found that the maximum distensibility of the arterial tube is manifested at lower and lower levels until at length, when contraction is quite abolished, the greatest expansion occurs with the first rise of pressure above zero. Thus the artery which showed increasing expansion up to 420 mm. in fig. 27, showed some days later (when its contraction was weak) maximum expansion t 40—60 mm., and later still immediately above 0.

Here it is necessary to remark upon the extraordinary persistence of a residuum of contraction in the arterial wall—in some cases even after signs of putrefaction are evident. Putrefaction begins in adherent blood, periarterial tissue, &c., much earlier than in the tunica media, and the survival of the latter is favoured by cleansing the artery after excision from blood and serum, and by removing connective tissue, &c., from around the vessel; still more by keeping the artery so prepared immersed in olive oil. Roy remarked that the elasticity curve of any given artery remains the same until putrefaction is far advanced—which is intelligible in view of what has just been stated.

In warm weather pieces of artery kept in a corked bottle, moistened with normal saline, may have a putrefactive smell in 2 days, while still contracted and showing a striking increase of contraction on stimulation by cutting, &c. In such circumstances, when arteries are kept for several days in defibrinated blood, it is important that the blood should be replaced by fresh blood from time to time.

Effects of Repeated Distension.—When a strongly-contracted artery has once been distended by high pressures, it is more easily stretched by a repetition of the rise of pressure, while the general character of the expansion remains the same—unless the first distension was very great. This is seen on comparing figs. 29 and 30.

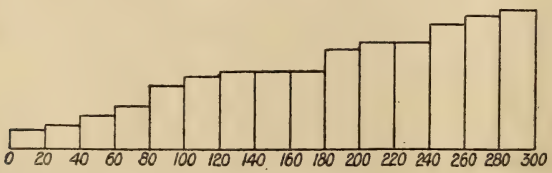


FIG. 29.—Carotid (ox), strongly contracted; 48 hours *p.m.* Length 14 mm.

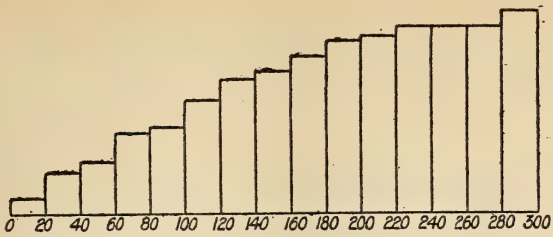


FIG. 30.—Same artery as in fig. 29; second elevation of pressure 24 hours after the first. The distensibility is seen to be much increased, though the general behaviour in giving enlarging increments of volume per unit rise of pressure is not changed.

In other cases where the artery is only slightly contracted, and the maximum distensibility is found at, say, 100—120 mm., a second rise of pressure causes the maximum distension at a much lower level—*e.g.*, at 40—60 mm., or, it may be, immediately above zero. (Compare figs. 28 and 31.)

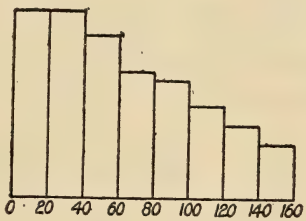


FIG. 31.—Second elevation of pressure in same portion of artery as in fig. 28, half an hour after completion of first experiment.

Such effects of repetition of the rise of internal pressure are manifested whether the second rise is induced very shortly after the first one (a few minutes) or after an interval of 24 hours or more. In the latter case the change may be more extensive than in the former—owing no doubt to the lapse of time leading to a diminution of the contraction present in the artery.

As regards the recovery of a contracted artery after it has been distended by internal pressure, this is often quite complete—at least when the artery is tested at a relatively early stage of *post-mortem* contraction, *e.g.*, within a day or two after death. The oil which has been driven out along the horizontal graduated tube when the artery expands under the increase of internal pressure returns quite to the starting-point; indeed it often moves back beyond its starting-point—showing that the artery has not only completely recovered from dis tension, but has contracted somewhat in volume from what it was at the beginning of the experiment.

This is a point of considerable significance and favours the view that the *post-mortem* contraction is a true persistent contraction, and not of the same nature as the rigor mortis of skeletal muscle.

The carotid of the sheep gave results essentially similar to those obtained from the ox, as is shown in the following figures. The graduated tube of the plethysmograph was of 1 mm. calibre; the expansion is expressed as before by the movement of the oil—stated in millimetres.

Pressure.—	0	20	40	60	80	100	120	140	160	180	200.	220	240	260.
Expansion.—		4	5	10	14	21	26	28	30	42	45	55	65	80.

Another sheep's carotid with less strongly-marked contraction showed the maximum expansion at 100—120.

Pressure.—	0	20	40	60	80	100	120	140	160	180.
Expansion.—		4	4	7	10	13	16	14	14	12.

A portion of the same artery when kept for some days in blood showed only slight evidence of contraction; its maximum distensibility was found to be between 40 and 60 mm. The artery was very much more distensible at low pressures.

Pressure.—	0	20	40	60	80	100	120	140	160	180	200	220	240	260
	280	300	320	340	360	380	400.							
Expansion.—		17	32	50	42	29	20	12	10	8	7	6	5	5
	5	4	4	3	4	3·5	3·5.							

Roy's well-known conclusion that the maximum distensibility of an artery was found at pressures corresponding, more or less exactly, to their normal blood-pressure, was probably based on the fact that he was dealing (unwittingly) with arteries in *post-mortem* contraction.

As regards the arteries in diseased animals and persons, Roy found that when defective nutrition, marasmus, &c., had been well-marked before death the arteries were found to be wider than normal, and their maximum distensibility was reached at pressures below normal—in some instances coming immediately above zero pressure.

Here there is good reason to believe that *post-mortem* contraction was small in amount or absent. I find that in emaciated worn-out old horses, *post-mortem* contraction is commonly very much less strongly developed than in more vigorous horses; and notably less so than in healthy oxen. In cases of exhausting disease in man there is reason to believe that *post-mortem* contraction is very slight in amount or absent.

It is to be noted that arteries show a striking general resemblance in (*a*) the expansion of the arterial tube when distended by internal pressure in each case, and (*b*) the elasticity of strips from the arterial wall when stretched by weights. This applies to both contracted and relaxed arteries.

Changes in the Length of Arteries during Variations in Internal Pressure.

We cannot at once predict the exact behaviour of an artery as regards changes in its length when subjected to variations in internal pressure from a study of the behaviour of longitudinally-cut strips of the arterial wall stretched by weights. For the relations of the tissues of the arterial wall are very different when they are in the form of a strip to what they are while forming a tube which is distended by internal pressure.

We have already seen that in an artery containing air at atmospheric pressure, the occurrence of contraction is accompanied by a lengthening of the tube, while relaxation is attended by a considerable amount of shortening.

An artery subjected to a considerable rise of pressure in its interior undergoes expansion in both its transverse and longitudinal diameters—in contrast to an excised artery at atmospheric pressure, which shortens when it widens from relaxation.

In the artery distended by internal pressure it is evident that the tendency to shortening which is associated with a marked increase in the transverse diameter of the tube is overborne by the stretching effect of the internal pressure.

The method I have employed for studying the changes in the length of an artery is as follows :—

A segment of artery was connected with a system of pressure bottles as already described. The portion of artery so prepared was placed in the vertical position, and the brass cannula tied into its upper end was rigidly fixed. The part of the wooden plug projecting from the lower end of the artery was then grasped by a small clamp attached to the lever of a Helmholtz myograph; the lever was directed at right angles to the smoked surface so that the writing point traced a vertical line. The pressure was usually raised 50 mm. Hg. at a time, beginning at zero, and it was usually kept at each level for 1 minute. The elongation is magnified 6 times by the lever. The pressure was raised to 300—400 mm. Hg.

Contracted Artery.—The total elongation in a strongly contracted artery is relatively small in amount; there is progressively increasing augmentation as the pressure is raised.

When the pressure is lowered the artery speedily returns to its original length—commonly indeed it becomes shorter than before.

A repetition of the process of raising the pressure sometimes causes the maximum elongation to occur much earlier than before, *e.g.*, with the rise from 50 to 100 mm., or from 100 to 150 mm. At other times there is simply a general increase in extensibility without the maximum elongation coming early (0—300 mm.). (See Fig. 32.)

FIG. 32.—Elongation of contracted artery with rise of internal pressure, 0—300 mm. Length 16 mm.

Relaxed Artery.—A relaxed artery tested in the same way shows a vastly increased elongation when the internal pressure is raised. The maximum elongation occurs with the first or second rise; sometimes the second and third are equal in extent. (Fig. 33.)

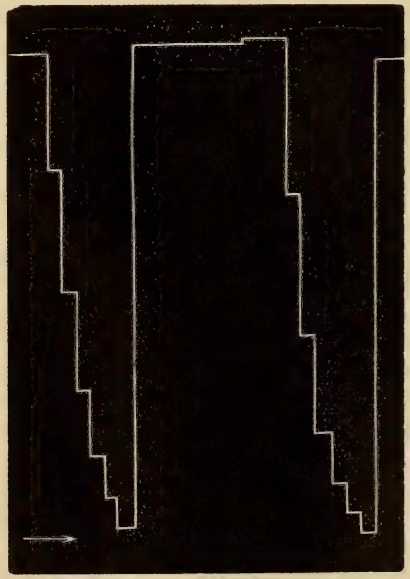


FIG. 33.—Relaxed artery which has been kept in blood for some days—till contraction had passed off. Length 21 mm.

A repetition of the rise of pressure shows an increased extensibility —especially for low pressures.

The artery was usually first tested in its contracted state, then relaxed by one or other of the methods mentioned, without any disturbance of the cannula or plug, and again tested in the same way as before. (Compare figs. 32 and 34.)

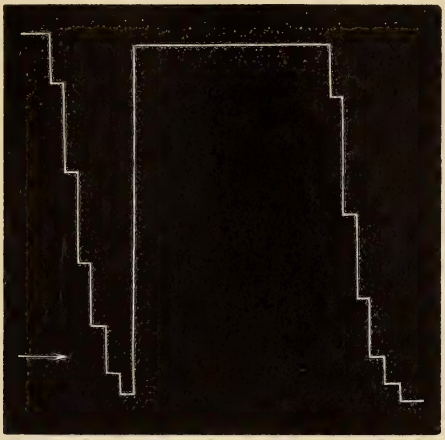


FIG. 34.—Artery, relaxed by keeping in warm chamber at 39° C. for 24 hours. Same portion of artery as was used (in contracted state) in fig. 32.

The relatively great tendency to elongation, even under low or moderate internal pressures, in a relaxed artery is important with regard to the tendency of certain arteries in the human body to become elongated and tortuous, apart from any recognisable structural change in the vascular wall. Elongation of the healthy temporal artery when relaxed is easily discernible even in young subjects; prolonged and frequently-recurring periods of relaxation tend to induce a more or less distinctly tortuous condition. Prolonged relaxation of the arteries of the uterus and mamma during pregnancy leads to similar changes in them.

Pulsatile Expansion of Relaxed and Contracted Arteries.—I have examined the pulsatile expansion of arteries when a rhythmical series of elevations and depressions of internal pressure is mechanically produced—in imitation of the rhythmical changes of pressure caused in a normal artery by the pumping action of the heart. This was tried when the pressure within the artery was at zero to begin with, or when a certain height of pressure had first been established; the apparatus for testing distensibility already described was used. Rhythmical compression of a rubber tube containing air in connection with that filling the interior of the artery was used to produce the pulsatile variation of pressure. The changes in the volume of the artery in response to the variations of pressure in its interior were shown by the to and fro movement of the oil in the horizontal graduated tube.

Relaxed Artery.—When there is zero pressure within the artery, to begin with a pulsatile rise of pressure of a certain amount causes an extensive pulsatile change in volume; *e.g.*, the oil in the graduated tube may move 15 mm. at each pulsation. (The tube is 3 mm. in calibre.)

When the pressure within the artery is originally 50 mm. Hg., the same rhythmical injection of air causes a much smaller expansion of volume, *e.g.*, 10 mm.

With higher pressures there is a smaller and smaller pulsatile expansion, *e.g.*, with an original pressure of 100 mm. the movement is 6 mm., with an original pressure of 200 mm. the movement is 3 mm.

Contracted Artery.—When a contracted artery is tested in the same way, the pulsatile expansion is very small in amount, and there is no very evident difference in the amount of pulsatile expansion at different (pre-existing) pressures. From the fact that (as has been described) a contracted artery is most distensible at a high pressure, one might expect that with a tolerably high pressure a further pulsatile rise would give larger expansion than when the pressure is at or near zero. But the rapid oscillations of pressure such as are now under consideration are much too brief in duration to have much effect; for the expansion of a contracted artery is slowly and gradually effected, and a rise of pressure, from whatever level it starts, has to last for some time if it is to exercise its full effect upon the arterial wall.

In accordance with this, in the intact arteries of men and animals there would be much less pulsatile expansion in a contracted artery than in a relaxed one; and in a relaxed artery expansion would be very much more extensive when the mean blood-pressure is low. Further, elongation would occur markedly in the relaxed artery as compared with the contracted one. And when a long stretch of artery is concerned the increase in length is very much greater than the increase in transverse diameter.

Illustrations of the above conclusions may be seen in healthy human arteries, the pulsatile changes in the relaxed temporal artery, also the extensive pulsation of arteries in inflamed parts, &c.

Various observers have described an apparent absence of transverse expansion at each heart-beat in arteries *in situ*. A. W. Volkmann* remarks that when an artery is laid bare in the living animal the only perceptible evidence of expansion of the vessel during pulsation is the tortuous form assumed by it at every pulse, and that the transverse expansion which (on other grounds) he believes to take place is inappreciable.

Lister† states that the arteries are not increased in diameter by the strokes of the powerful cardiac pump—"The surgeon when tying a

* 'Haemodynamik,' 1850.

† 'Brit. Med. Journ.,' 1879, vol. 1, p. 924.

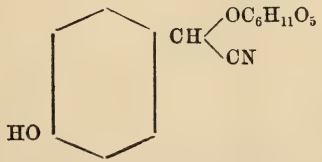
large arterial trunk in its continuity does not find, on clearing the vessel of its sheath with the point of his knife, that he is dealing with a body that swells at every pulse, but with one of unvarying dimensions." Experimenting on the metacarpal artery (horse) with the circulation going on, he found that transverse measurements with suitable callipers showed no change as long as the limb was kept in any one position.

"Cyanogenesis in Plants. Part II.—The Great Millet, *Sorghum vulgare.*"* By WYNDHAM R. DUNSTAN, M.A., F.R.S., Director of the Scientific Department of the Imperial Institute, and T. A. HENRY, D.Sc. Lond. Received April 24,—Read May 15, 1902.

(Abstract.)

The authors have investigated the nature of the poison contained in the young plants of *Sorghum vulgare*, the Great Millet or Guinea Corn (the *Juár* of India or *Dhurra shirshabi* of Egypt). This plant is cultivated in tropical countries for the sake of the seed, which is important as a food grain. The young plants have proved fatal to animals, especially in Egypt, where the attention of the authors was directed to the subject by Mr. E. A. Floyer, of Cairo, who has kindly provided the material required for the investigation.

The authors show that the young plant, but not the seeds or old plants, when crushed with water furnishes prussic acid (about 0·2 per cent. of the dried plant). The acid is not present in the free state, nor is it produced by acting on the plant with boiling water or with alcohol. The production of the poison is due to the action of a hydrolytic enzyme, apparently identical with the emulsin of bitter almonds on a cyanogenetic glucoside which has been named "dhurrin," from the Arabic name for the plant, "dhurra." This glucoside has been proved to be derived from parahydroxymandelic nitrile by the association of the residue of one molecule of dextrose. Its formula is therefore $C_{14}H_{17}O_7N$,



* The authors' previous paper, entitled "The Nature and Origin of the Poison of *Lotus arabicus*" ('Proceedings,' vol. 67, 1900, p. 224; vol. 68, 1901, p. 374; and 'Phil. Trans.,' B, vol. 194, 1901, p. 515), is to be regarded as Part I of this series.

Dhurrin crystallises well, and is soluble in both water and alcohol. When hydrolysed by emulsin or by dilute acids it is converted into parahydroxybenzaldehyde, dextrose, and hydrocyanic acid according to the equation $C_{14}H_{17}O_7N + H_2O = C_7H_6O_2 + C_6H_{12}O_6 + HCN$. When warmed with alkalis, dhurrin is resolved first into dhurrinic acid and ammonia This acid subsequently undergoes further hydrolysis when warmed with dilute hydrochloric acid, being converted into parahydroxymandelic acid and dextrose (1) $C_{14}H_{17}O_7N + H_2O = C_{14}H_{18}O_9 + NH_3$, (2) $C_{14}H_{18}O_9 + H_2O = C_8H_8O_4 + C_6H_{12}O_6$.

The identity of the parahydroxymandelic acid was established by its synthesis from the cyanhydrin of parahydroxybenzaldehyde.

Dhurrin differs from the other two known cyanogenetic glucosides, the amygdalin of bitter almonds and the lotusin found by the authors in *Lotus arabicus*, in being derived from dextrose and not from maltose.

The authors point out the protective purpose served by the existence of the cyanogenetic glucoside in the young plant.

The authors intend to fully investigate the several problems which are raised by the occurrence of cyanogenetic glucosides in plants.

They are at present engaged in examining several other plants which have furnished prussic acid, among them being *Manihot utilissima*, *Linum usitatissimum*, *Lotus australis*, and *Phaseolus lunatus*.

"On the Continuity of Effect of Light and Electric Radiation on Matter." By Jagadis Chunder Bose. Communicated by Lord Rayleigh, F.R.S. Received April 18,—Read June 20, 1901.

Introduction.

Though the theory of coherence gives a simple explanation of many cases of diminution of resistance in a mass of metallic particles under electric radiation, yet there are other cases which are not explicable by that theory. If coherence be due to electric welding, it would follow that *all* sensitive particles would exhibit a permanent diminution of resistance; in other words, the action should be non-discriminative and there should be no self-recovery. In my previous paper,* however, I have shown that the effect of radiation is by no means non-discriminative. To the contrary, while its effect on the positive class of substances, *e.g.*, Mg.Fe.Ni, is a diminution of resistance, it acts on the negative class, *e.g.*, K.Ag'.Br.I, in a precisely opposite way, that is to say, it produces in these an increase of resistance. Further, the conduc-

* "On Electric Touch and Molecular Changes produced in Matter by Electric Waves," 'Roy. Soc. Proc.,' vol. 66.

tivity change is not always permanent, since several substances are known which quickly recover and attain their original conductivity on the cessation of radiation, as if a force of restitution were called forth to restore them to their original condition.

I was thus led to suppose that the effect of radiation is to produce a state of molecular strain. Evidences will presently be adduced which it is hoped will furnish proofs as to the correctness of this view.

If a substance is molecularly distorted by the action of an external agent, we may naturally expect that there would be produced changes in the physico-chemical property of the substance. As a familiar example, take the case of phosphorus changed from the yellow to the red variety by the action of visible radiation. We find that in the allotropic condition of red, the phosphorus has become less active chemically, insoluble in CS_2, and of higher specific gravity. Similarly its other properties, such as its elasticity, its position in the voltaic series, its electric conductivity, &c., are likely to undergo a corresponding modification. The *same* molecular phenomenon, seen from different aspects, may thus appear to be diverse. Looking from an electric point of view we do find that the conductivity of red phosphorus is greater than that of the yellow variety. We thus see the possibility of measuring the molecular change by measuring the correlated variation of any of the properties described above. The choice of a particular method will be governed by special convenience under given conditions.

If the above view is correct then it would be possible to detect the effect of molecular strain due to visible or invisible radiation by the following more or less delicate methods. It is to be borne in mind that the effect of radiation is almost confined to the skin or outer layer of the substance.

(1.) Method depending on the variation of the adhesive or cohesive power of a substance, *e.g.*, in a daguerreotype plate the mercury vapour adheres in preference to the light-impressed portions only. Images may in a similar manner be developed by water vapour. Under the action of electric radiation, particles of certain metals are known to stick together. But this is by no means universal.

(2.) Method depending on the variation of chemical activity undergone by the strained substance, or the method of photographic development. The acted and unacted portions are differently attacked by the developer. The action is not altogether independent of the effect described below.

(3.) Method depending on the variation of electric potential, by which an E.M.F. is produced between the acted and unacted portions of a substance originally iso-electric.

(4.) Method depending on the conductivity variation produced by the strain.

In the following investigations I shall employ specially the two last methods, and hope to demonstrate the fundamendal unity of effects of visible and invisible radiation on matter. The subject is very extensive, and I propose to deal with it, as briefly as is compatible with clearness, in the three accompanying papers :—

I. "On the Continuity of Effect of Light and Electric Radiation on Matter." In this paper various experiments will be described and results given, which can only be explained on the supposition that the observed effects are due to strain.

II. "On the Similarities between Mechanical and Radiation Strains." If the effects as described in (1.) are really due to strain, then similar results might be brought about by artificially producing strain regarding which there can be no possibility of doubt, for instance, strain by mechanical means. In this paper I shall show the remarkable parallelism between those two classes of phenomena throughout an extensive range.

III. "On the Strain Theory of Photographic Action." Having shown the strain effect due to light, I will show how some of the most obscure phenomena in photography receive a simple explanation on the above theory.

Effect of Electric Radiation.

1. *Method of Conductivity Variation.*

This method is specially well suited for studying the effect of electric radiation on discontinuous particles. For the action of radiation being one of surface, the larger the area of this the greater is the result, and in loose particles the effective surface is very much enlarged. In this case, again, the effective total resistance of the mass of particles being due to resistances of *surface* contacts, any change in the property of surface layers will greatly modify the total resistance. In a continuous solid, on the other hand, only a comparatively thin molecular layer on the surface is acted upon; but this has little effect on the conductivity of the mass in the interior, protected by the outer conducting sheet. A slight conductivity variation can, however, be detected if the continuous solid takes the form of an extremely thin layer. I shall presently show that for the detection of molecular strain in a continuous solid the electromotive variation is the more suitable.

I have before said that in the positive class there is produced a diminution, and in the negative an increase of resistance. These opposite properties at first seem difficult to understand, but about their reality there can be no doubt. In another paper to be shortly communicated, I shall give an account of an independent inquiry in which the positive, the neutral, and the negative classes of substances are differentiated by their characteristic curves. In the paramagnetic

and diamagnetic classes we also come across characteristic differences, though these divisions do not coincide with the classes giving the positive and negative electric touch. There is, however, one similarity; for just as the paramagnetic effect is more intense, so is the conductivity variation associated with the positive, generally speaking, much stronger than the effect on the negative class. From the strongly positive substance like Fe Mg to the pronounced negative like K there are numerous gradations. In silver we have a material which is almost on the line of demarcation; it passes easily from one condition to another under the influence of external circumstances.

2. *External Influences.*

If the response, positive or negative, is really an expression of some changed molecular condition, we may expect it to be modified not only by the chemical nature of the substance, but also (1) by the previous history of the substance, (2) by the temperature, (3) by pressure.

Influence of previous History.—As regards the first, I have already shown in my previous paper* that a substance strained by radiation often exhibits opposite or reversal effects. Freshly powdered particles often show erratic results, but the effects become consistent after annealing; this often increases the sensibility also, by increasing the molecular mobility. Fresh particles are sometimes found to exhibit very little sensitiveness. At first I thought that this might be due to some kind of fatigue; the following, however, showed that such could not be the case. I found that in these cases the sensibility was increased by subjecting the substance to strong radiation or even by passing a few induction shocks. The increase of sensitiveness thus produced appears to be due to the removal of molecular sluggishness. The improvement in sensitiveness often obtained by shaking of the particles is no doubt due to the same cause. In the various types of molecular receivers, whether responding to electric radiation, light, or mechanical vibration, the same effect is noticed. Generally speaking, the receiver in the first place improves gradually with working. But as it gets overstrained it exhibits fatigue.

Influence of Temperature.—As regards temperature, I have in many instances found that on excessively cold days some receivers exhibit a diminution of sensibility, removed by warming. Several effects which were very strongly exhibited in the warm climate of India, I found to be much diminished here. Cautious application of heat often increases, not only the sensibility, but also the power of self-recovery. But excess of temperature produces erratic behaviour by causing violent molecular disturbance.

* *Loc. cit.*

Influence of Pressure.—Pressure also has pronounced effect on molecular response. Moderate increase of pressure increases the sensibility, but too great an increase may cause loss of sensibility. In substances which are nearly neutral, pressure variation may even cause reversal of response.

Again, the same receiver may, owing to some molecular modification, exhibit a response opposite in sign to the normal. But subjection to this continued stimulation of radiation, generally speaking, converts the abnormal response into normal. Exactly parallel instances will be noticed in the case of response to mechanical stimulus and to light.

We thus see how the response is dependent on the molecular condition, and how a change of this condition may even give rise to a reversal of response, say, from a diminution to an increase of resistance. The nature of the chemical substance, the molecular condition, the intensity and duration of radiation, the pressure, the temperature, and even the electromotive force used for detection, are the factors which are instrumental in the modification of the final response. I have already shown* how the increasing effect of continuous radiation may produce molecular reversal. There may thus be one or more reversals. It is probable that the other variables may also produce similar reversals.

3. *Recording Apparatus.*

In the following investigations, the electrical effects, either the conductivity or the electromotive variations, due to external disturbances of various durations, have to be observed. It is also necessary to note the time-relations of the after-recovery from these effects. The conductivity and electromotive variations can be deduced from the observed varying deflections of the galvanometer. When the variations are rapid, the observation requires great alertness and is very fatiguing. This difficulty is still greatly enhanced when simultaneous time-observations have to be taken. It thus becomes necessary to have at least two observers; the process of observation is made slow and tedious, and the accumulation of results by this method is very tardy. But in the apparatus now to be described, the mode of procedure has been very much simplified, affording facilities for quick and highly accurate observations.

The apparatus is a modified railway myograph. (See fig. 1.) The moving platform carries squared paper (divided into $\frac{1}{10}$ inch) on which the record is made. The platform moves uniformly by clockwork, and the rate of travel of the paper may be roughly adjusted by means of different-sized pulleys, or more finely by the clockwork governor. The usual rate is 1 inch in 30 seconds, and one small division of the paper measured horizontally is thus equal to 3 seconds.

* *Loc. cit.*

But very much quicker or slower rates may easily be obtained by means of the different-sized pulleys. The spot of light from the galvanometer is thrown down on the paper by an inclined mirror. The movement of the galvanometer spot takes place at right angles to the direction of motion of the paper. There is a guide rod at right angles to the motion of paper, along which the recording pencil is moved. The excursion of the galvanometer spot can thus easily be

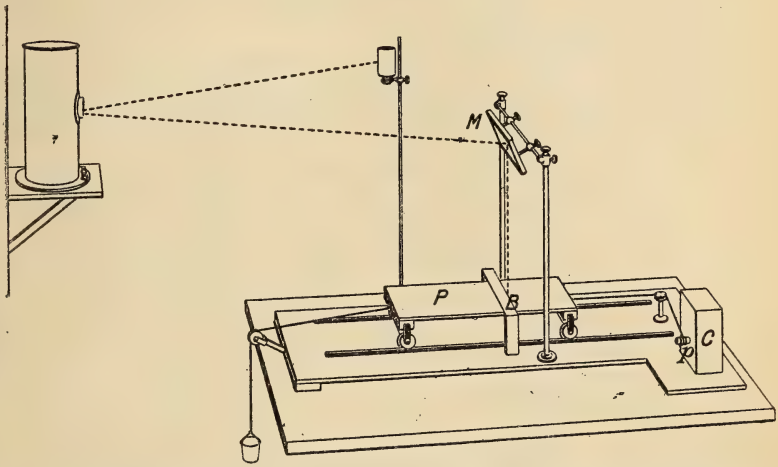


FIG. 1.—The Recording Apparatus. P is the platform moving on rails and carrying the squared recording paper. B is the guide bar. C, the clockwork. P, the pulley. M, the mirror to reflect the galvanometer on the platform.

followed with a pencil, and it is quite easy to do this, when the fluctuation period is more than 2 seconds. In the experiments to be described, this period varied from 2 seconds to several minutes. A curve is thus directly obtained, with conductivity or electromotive variation as ordinate, and the time as abscissa. The curves given in the accompanying papers are exact copies from the direct records.

4. *Transition of a Molecular Receiver from Non-recovering to Self-recovering Condition.*

When a substance is strained by radiation there is produced a sudden variation of conductivity. The substance will recover from the strained condition (1) if it has not been overstrained by an excessive stimulus, or (2) if its electric elasticity be very great. I have found in general that on careful adjustment of a receiver there is a tendency to self-recovery if the intensity of incident radiation is not too strong. In the case of substances which are, electrically speaking, highly elastic, such as K, there is an automatic recovery even when

the stimulus is strong. The difference exhibited by various substances as regards self-recovery is one merely of degree. I give below typical cases which exhibit the gradual transition from so-called non-recovery to complete recovery.

The galvanometer used is a dead-beat D'Arsonval, in which the period taken for the maximum excursion and the return to zero from that position is less than half a second.

In these experiments the receiver was appropriately fixed on a heavy base. This rested in turn on a steady pedestal with one or two sets of pneumatic tyres interposed.

Positive Type.—In fig. 2 (*a*) is shown the effect of radiation on Fe_3O_4 when cold. Only the upper portion of the curve is given; the flash of radiation produced a deflection of the galvanometer of sixty-four

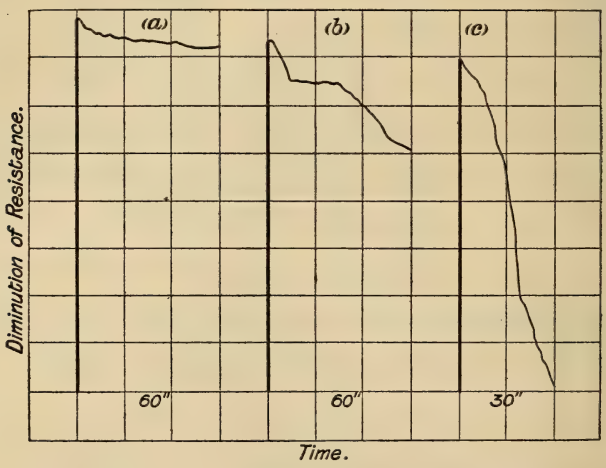


FIG. 2.—Different stages in the evolution of a Molecular Receiver from a non-recovering to a self-recovering condition. The substance used is Fe_3O_4, representing positive type, which exhibits a diminution of resistance under electric radiation. (*a*) the so-called non-recovery, really a case of very slow recovery; (*b*) the same slightly warmed, exhibiting a partial and arrested recovery; (*c*) the same with increased molecular mobility, recovery in 30″. In this and the following cases, thick lines represent the effect of radiation, thin lines represent the recovery.

divisions. It will be observed that it had recovered to the extent of three divisions in the course of 60 seconds; if the rate of recovery had been uniform, there would have been complete recovery in about 21 minutes; but in the later stages, as we shall see, recovery is rather slow. In (*b*) is shown the effect of increased molecular mobility due to cautious warming. Now there was semi-recovery in 60 seconds; the quickness of recovery went on improving, and after a while the recovery was completed in 30 seconds.

Negative Type.—Fig. 3 exhibits the effect on the negative type as exemplified by Sn and Pb coated with Br. It appears that in these, warmth is favourable to quick recovery. For in India with Pb coated with Br the self-recovery was obtained with the greatest ease. Here a more careful adjustment was necessary. At first the flash of radiation produces a sudden increase of resistance, from which there is no im-

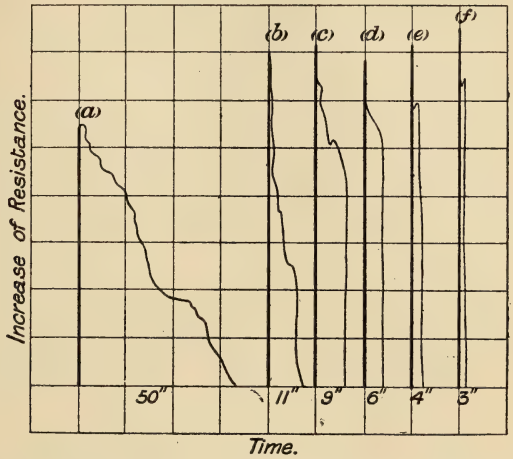


FIG. 3.—Self-recovering Receiver exhibiting negative *touch* (increase of resistance under radiation). (*a*), (*b*), (*c*), and (*d*) are the different stages of quickness of recovery (from 50″ to 6″) in a brominated tin receiver; (*e*) and (*f*) are the records of a brominated lead receiver.

mediate recovery, but an inspection of the galvanometer spot at once makes it evident that some internal struggle has been going on; the spot trembles, and then after awhile the internal friction is overcome, and the substance suddenly recovers. But soon after this preliminary stage the recovery becomes perfectly automatic and instantaneous. Each flash of radiation produces a responsive galvanometer twitch, *immediately* followed by recovery.

Some of the stages are well seen in the curves given. In (*a*) the recovery takes place in 50 seconds; in (*d*) in only 6 seconds. (*e*) and (*f*) show recovery in lead coated with bromine. The recovery gradually became very quick, from 4 seconds to 3 seconds; after that it was too quick for record.

In all the above cases it will be noted that the curve of recovery is convex to the abscissa; that is to say, it is at first very rapid, but in the later stage it becomes slower.

5. *A Self-recovering and Metrical Receiver.*

But the most perfect type of self-recovering receiver that I have succeeded in constructing was made of the strained variety of silver described in my previous paper. I there showed that this variety of silver exhibits, under the action of radiation, an increase of resistance. I had with me a portion of this variety prepared more than a year ago, and it is probable that time had improved its quality. I made with it a receiver by having about 3 mm. thickness of the powder between two electrodes, one of which, by means of a micrometer screw, could be made to produce a gradual compression. The applied electromotive force was 0·4 volt, and the resistance of the receiver was equal to 20 ohms. The receiver showed the usual increase of resistance at first, with a tendency to self-recovery. In about half an hour it began to exhibit the most perfect self-recovery, and for the next 3 hours of continuous work it went on giving an extraordinary consistency of response.

A short rod was the source of radiation throughout these experiments. The intensity of incident radiation was varied by changing the distance of the radiator. In fig. 4 are given responses to individual flashes at distances of 40 and 15 cm. It will be seen that the

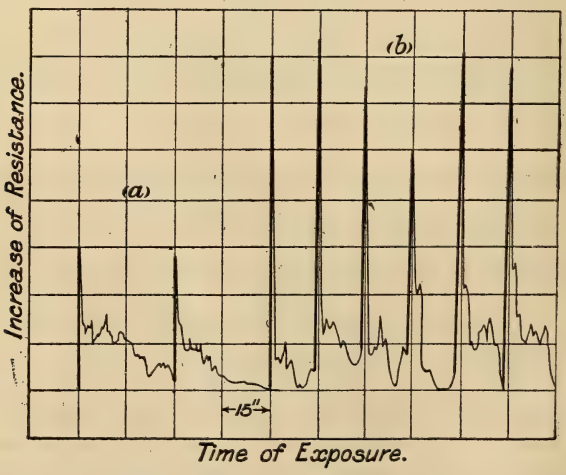


Fig. 4.—Transient increase of resistance in an Ag′ receiver due to single flashes of radiation. In (*a*) the radiator was at a distance of 40 cm.; in (*b*) the distance was 15 cm. Thick lines represent the effect of radiation, thin lines represent the recovery.

effects are very consistent, the occasional variation being probably due to certain oscillatory sparks not having been as efficient as the others.

Certain Analogies with the Phenomena of Phosphorescence and Thermo-

luminescence.—Another remarkable phenomenon will be noticed in the recovery curve. It will be seen that the complete recovery is effected after a series of minor oscillations. In other words, there seems to be an after-vibration which persists for a time in substances subjected to radiation. This is very suggestive with reference to a not altogether different after-effect of light in the fluorescent and phosphorescent bodies. In the case of Ag′ receiver, owing to its molecular mobility, the recovery is automatic. But in the case of so-called non-recovering substances, the strain persists for a considerable time, but the recovery may be hastened by removing molecular friction through gentle heating. In connection with this, I will quote an interesting observation described in my paper previously mentioned.* In an iron receiver strained by radiation there was quick recovery after heating, but "on careful inspection a slight oscillatory movement of the galvanometer spot was noticed during the process." Here the strain produced by radiation which remained latent was released by heat. In connection with this, one cannot help being reminded of the phenomena of thermo-luminescence, where the strain effect of light remains latent till set free by the application of heat.

Effect of Continuous Radiation.—Still more interesting are the superposed effects of a series of flashes of radiation. The first flash produces a certain molecular distortion, attended with conductivity variation, from which it will tend to recover. I may anticipate certain results which will be described later, in saying that the force of restitution increases with increasing distortion. Now if, before the substance has recovered from the first shock, a second flash be superposed, it will produce further distortion; but the effect will not be quite so strong, inasmuch as the force of restitution is increasing. Thus a series of superposed flashes will produce a limiting effect, which is kept balanced by the force of restitution. If the intensity of radiation is increased, the balancing position will be different.

It also appears from the result of other experiments that the after-effect persists for a little longer time when the stimulus is stronger. I shall show that this is the case when interpreting the curves of effect.

In fig. 5 are shown the effects of rapidly succeeding flashes of radiation caused by the spring vibrator of a Ruhmkorff's coil. In (*a*) the radiator was kept at a distance of 40 cm., and the radiation was continued for 15 seconds, after which 15 seconds was allowed for recovery. A longer time would have allowed a more complete recovery, but this would have entailed a great loss of time in the long series of experiments contemplated. The recovery is thus seen to be partial, the return curves not exactly reaching the original starting position. It will be seen from the effect at 40 cm. that even

* *Loc. cit.*

there we have partial recovery between quickly recurring flashes, and there is thus observed a fluctuation about the balanced position. Now

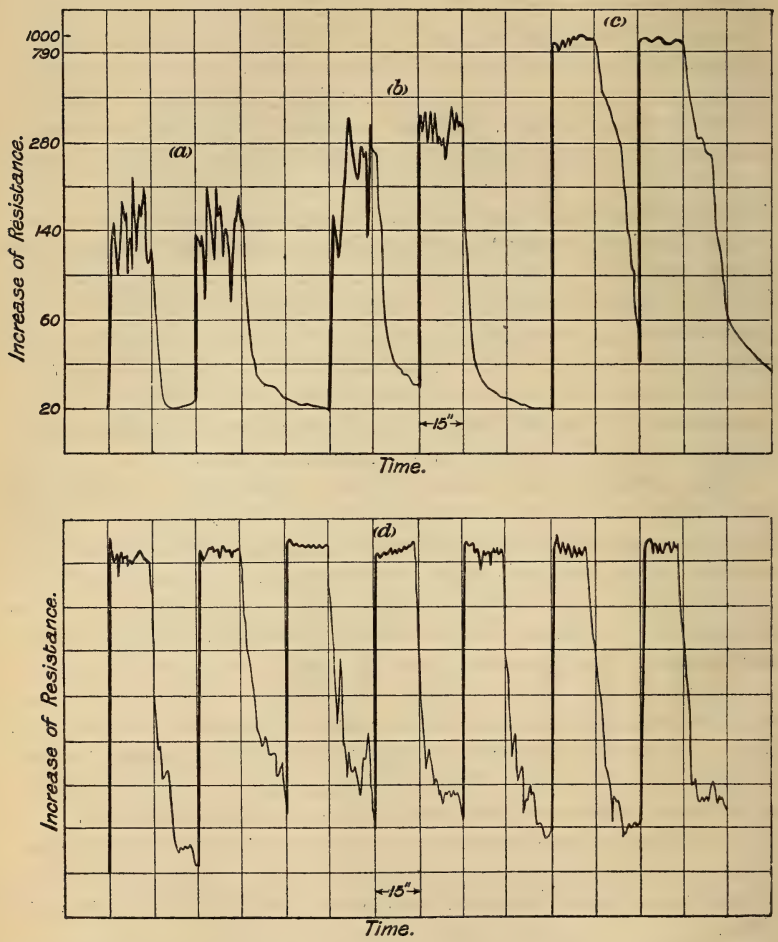


FIG. 5.—Variation of resistance (deduced from diminution of galvanometer deflection) in Ag′ receiver produced by electric radiation lasting from 15″. Distance of radiation in (*a*) = 40 cm.; in (*b*) = 25 cm.; and in (*c*) = 15 cm. In (*d*) is given a series of curves taken after half an hour with the radiator at a distance of 15 cm. The numbers on the left side of the upper curve indicate the absolute value of resistance.

when the intensity of radiation is increased by decreasing the distance of the radiator to 25 cm. (see *b*), the strain effect persists for a little longer time, and the flashes arrive before the substance can recover to any extent; thus there is less fluctuation in the balanced position.

But when the radiator was placed at a distance of 15 cm. (see (*c*)), the fluctuations almost disappeared, and the galvanometer deflection was held rigid as long as the radiation was kept on; in fact, we have here an effect which physiologists describe as "tetanic." On the cessation of radiation there was *immediate* recovery. It will be noticed how extraordinarily consistent are the succeeding values of response. The resultant effect being due to the additive effects of numerous flashes, an occasional failure of an individual flash has little or no importance. The series of responses in (*d*) was taken after half an hour, and it will be noticed how very consistent they are among themselves, and how similar to those in (*c*), showing that even after half an hour's continuous work there had been no fatigue, with the attendant change of sensibility.

Relation between the Intensity of Radiation and the Conductivity-variation.—The resistance of the receiver being not very large, the external resistance of the shunted galvanometer and of the cell are not negligible in comparison, and the variation of deflections is, therefore, not proportional to the variation of resistance. To interpret the absolute values of the deflections, a resistance box was substituted for the receiver, keeping the rest of the circuit just as before. In this way the absolute values of the resistances corresponding to particular deflections were found. Some of these are given on the left-hand side of fig. 5.

The galvanometer deflections, when the radiator was at distances of 40, 25, and 15 cm., were 23, 33, and 42 divisions respectively. Owing to the comparative steadiness of the last two deflections there is no uncertainity about them; but on account of the fluctuation in the deflection when the radiator is at a distance of 40 cm., it is difficult to find the exact value of the deflection; the mean of the various deflections gives twenty-three divisions. The absolute values of resistances corresponding to these deflections are 180, 380, and 1020 ohms. The original resistance being 20 ohms, the *variations* due to the different radiation intensities are 160, 360, and 1000 ohms.

The intensities of radiation at the above distances may approximately at least be taken as proportional to $\frac{1}{40^2}$, $\frac{1}{25^2}$, $\frac{1}{15^2}$, or as 14 : 36 : 100. The corresponding molecular effects as measured by the increase of resistance are found to be as 16 : 36 : 100.

It will thus be seen how accurately the indications of the Ag′ receiver measure the intensity of radiation. Further progress in the study of different phenomena connected with electric radiation has been seriously hampered owing to the want of means for measurement of intensity of electric radiation. But this difficulty, as will be seen from the above, is not insuperable.

The strict proportionality of molecular effect can only be taken as

true through a limited range. From the results of various experiments, into the detail of which I can not at present enter, it appears that, generally speaking, the curve of response (with molecular effects as ordinates, and the intensities of disturbance as abscissæ) is not a straight line. It is at first slightly convex, then straight, and in the last part concave. It is only in the second part that the curve is approximately straight.

In considering the effect of electric radiation in varying the conductivity of the particles, we have to bear in mind that no explanation can be regarded as complete, unless it explains not only the diminution, but also the increase of resistance; also the phenomenon of automatic recovery and of the opposite effects which are exhibited by the same receiver under different molecular conditions. The increase of resistance of the Ag′ receiver and its instantaneous recovery are directly opposed to the theory of coherence.

The state of balance between the distortion produced by radiation and the force of restitution on the one hand, and the different equilibrium positions with different radiation intensities on the other, point to the effect being due to some strain produced by radiation.

Fatigue of the Receiver.—I wished to trace the gradual appearance of fatigue in the Ag′ receiver, and for this purpose kept it acted on with slight intermissions for nearly 3 hours. At the end of that time it began to show unmistakable signs of fatigue. Fig. 6 shows the

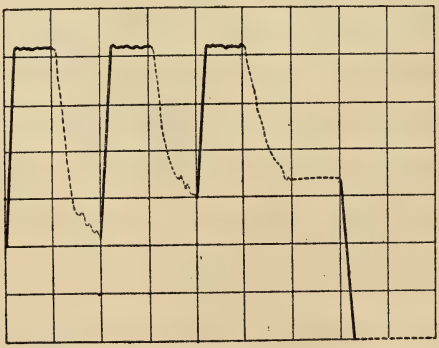


FIG. 6.—Fatigue and reversal in the Ag′ receiver. Thick lines represent the effect of radiation, and dotted lines the recovery. Observe in the first three records the incomplete recovery with growth of fatigue. In the fourth, there is produced a reversal (a diminution of resistance instead of the normal increase).

effect when the radiator was at a distance of 20 cm.; the deflections were now only twenty-one divisions, whereas before this the deflection was thirty-three divisions with the radiator at the increased distance of 25 cm. Formerly the recovery commenced immediately on the cessa-

tion of radiation, now there was a short period of hesitation and then it began to recover somewhat slowly. The extent of recovery also grew less and less, and at last the receiver suddenly exhibited the reversal effect, by showing a *diminution* of resistance.

A parallel instance under the continued action of light will be noticed later on.

6. *Phenomena of Reversal.*

(a.) *Reversal due to Sub-normal Intensity of Stimulus.*—Another very curious phenomenon met with is the opposite effects of radiation below and above the critical intensity. Thus I have shown* that, whereas under certain conditions the effect of radiation of moderate intensity on As is to produce an *increase* of resistance, the effect of feeble intensity of stimulus is to produce a diminution. Exactly parallel, though opposite, effects are sometimes seen produced in the positive class of substances. This result is certainly very curious, but I will show later on that exactly similar effects are produced under mechanical stimulus.

Possibly connected with the above is the following: When a receiver is subjected to radiation of moderately strong intensity, I have often noticed a short-lived negative twitch immediately followed by the normal response. This is probably due to the fact that it takes some time for the sensitive substance to absorb the whole amount of incident radiation. The first moiety absorbed may thus fall below the critical intensity, hence the preliminary negative twitch, while a little later, on the absorption of the whole amount, we get the normal response. Thus under the continued action of radiation, the response curve exhibits a negative twitch at the beginning followed by the normal positive effect (see also fig. 17).

(b.) *Reversal due to Overstrain.*—In addition to the above, I have also shown* that reversal effects are produced by overstrain due to the continued effects of radiation; and these reversals may be partial or complete. This depends on the nature of the substance, and also on the adjustments. I give below a curve (see fig. 7) for Fe_3O_4 under continuous radiation, where, after the maximum effect was reached, there was a distinct trend towards reversal.

Under certain conditions, we may thus have in a positive substance an increase of resistance or negative effect under feeble radiation; this is specially seen when the receiver had been undisturbed for a long time, and the substance had undergone certain unknown molecular modification. Annealing makes the responses normal; or under moderately strong intensity, the abnormal negative response becomes changed into positive, to be again reversed (or tend to be reversed) under strong and continued action of radiation. All these peculiarities

* *Loc. cit.*

will also be found characteristic of other receivers responding to the stimulus of light or of mechanical vibration.

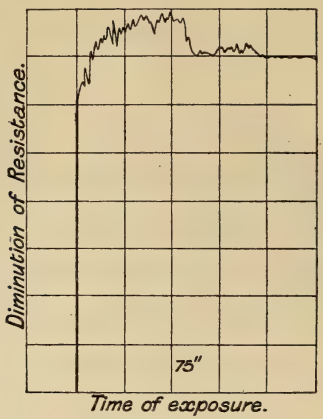


FIG. 7.—Tendency towards reversal under the continued action of radiation on Fe_3O_4 receiver. Thick line shows the immediate effect, the thin line the continued effect of radiation. In other cases the reversal is complete.

7. *Physical Nature of the Change.*

From the fact that the conductivity variation above described takes place in platinum and other noble metals, and the further fact that the action goes on even when the substance is kept immersed in a protecting medium such as naphtha, it would appear that the observed effect is primarily due to physical strain. By chemical action we generally understand an irreversible operation. But in the cases of complete self-recovery exhibited by various substances, we have an automatic return to the original condition. It should, however, be borne in mind that, as a result of the strain, the chemical activity of the substance may be changed, and if the strained substance happen to be immersed in a medium for which under strain it has a relatively stronger attraction, there may then be a chemical action. But this would be merely a secondary effect.

8. *Electromotive Variation produced by Electric Radiation.*

I said that if radiation causes molecular strain, there might be produced a difference of potential between the acted and unacted portions of a substance. A voltaic cell could be made with two plates of similar substance; there would then be no P.D. between the two. But on exposing one to the effect of radiation, a difference of potential may be established between the acted and unacted plates. The differential effect, if it exists, could then be detected by a galvanometer

or an electrometer. In the carrying out of this method there are, however, many practical difficulties. First of all, in making a voltaic combination, some kind of electrolyte is necessary, but unfortunately all electrolytes are opaque to electric radiation. This difficulty could, however, be obviated to some extent by taking an electrolyte which is almost a non-conductor (*e.g.*, amylic alcohol) so as to be partially transparent to electric waves. But the second difficulty is far more serious. Owing to the diffuse action of the comparatively long electric waves it is impossible to shield one plate while exposing the other. If both the plates are equally acted on, there would then be no electromotive *variation*. From these considerations any attempt to detect the effect of electric radiation by electromotive variation appeared to be hopeless. It was only after the conclusion of another line of investigation on the electromotive variation produced by mechanical stimulus that I learnt that the effects of the same stimulus on two pieces of the same metal, forming a voltaic element, are different if the molecular conditions of the two are not originally the same. Under such a condition a P.D. exists between the two, and stimulation of both causes a variation of the existing electromotive force.

From the similarities of the effects of radiation and mechanical strain (see the following paper) I was convinced that with radiation, too, I would get unequal effects on the two plates having a slight initial electromotive difference. The effect of radiation would then be to produce a *variation* of the original electromotive force. And if the effects are at all parallel to those observed in the conductivity variation method (as diminution or increase of resistance) we may likewise expect to find a diminution or increase of electromotive force.

In carrying out experiments to verify the above suppositions, I soon found my anticipations to be fully justified. I at first made a cell by taking two varieties of silver. A piece of cotton wool moistened with amylic alcohol was placed in a glass tube. Ag and Ag′ were placed on opposite sides of the moistened cotton; this formed a voltaic element. Two electrodes compressed the powder, till a current was observed to flow. The amylic alcohol acted as the electrolyte. Very careful adjustment of pressure is necessary, as in the case of receivers for exhibition of conductivity variation. In order that the effect observed might be purely due to electromotive variation and not to the variation of conductivity, the cell was connected with a capillary electrometer. On allowing electric radiation to act on the cell, there was produced a variation of electromotive force; continued radiation even produced a reversal. The electric radiator—a rod used in the previous experiments—was also used in this case: owing to the opacity of the electrolyte the intensity of radiation has to be strong. The radiator was placed at a distance of 6 inches from the cell.

I give below the results of three other experiments with different combinations :—

	Original E.M.F.	New value after the action of radiation	Percentage reduction.
I.....	1·26 V.	1·15 V.	9
II.....	0·39 V.	0·312 V.	20
III.....	0·065 V.	0·039 V.	40

I was now desirous of obtaining a continuous record of electromotive changes produced by the continued action of electric radiation. For this purpose I used a galvanometer.

A cell was made, in the way previously described, with magnesium powder. Owing to some differences in the two portions of the powder there was an initial P.D. of 0·042 V. between the two electrodes. I now had the cell balanced by the potentiometer method, a sensitive galvanometer (with an interposed high resistance) being used as the detector of electric variation. Fig. 8 (*a*) shows the deviation from the balanced position by radiation which nearly reduced the potential difference to half its original value. By tapping, the original P.D. was restored, and a second experiment (see (*b*)) gave almost identical results.

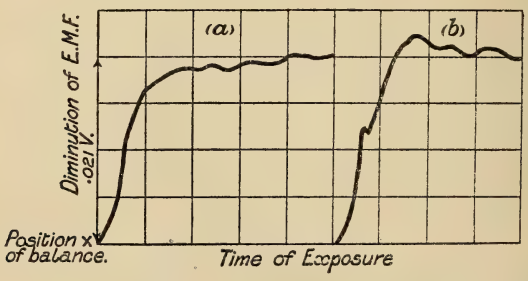


FIG. 8.—E.M. variation in a Mg receiver. The original E.M.F. was 0·042 V. This was reduced to 0·021 V. by electric radiation.

It is thus seen that the curve of electromotive variation due to radiation is similar to that obtained by the conductivity variation method.

It has already been shown that when the range of electric elasticity of the substance is not narrow, or when the strain is not too great, there is a recovery. That on subjecting the substance to the continued action of radiation there is a limiting effect; that too long continued action tends to produce an electric reversal; that too feeble an intensity may also produce a reversed effect. We shall now study whether visible radiation produces similar effects.

Effect of Light.

The molecular effect due to visible radiation may as in the previous case be detected by the method of conductivity or electromotive variation. That light does produce conductivity variation is seen in selenium. I have also succeeded in detecting the effect of light in producing variation of contact resistance. One and the same receiver responded in the same way when alternately acted on by visible and invisible (electric) radiation. It is, however, difficult to discriminate the effect of light from that due to the rise of temperature. That the effects observed were not solely due to temperature was evident from the fact that there was a tendency towards reversal, and that the same receiver which normally exhibited a diminution of resistance exhibited an increase of resistance when it underwent a molecular modification. The peculiarities of this universal radiometer was in every way similar to those of detectors for electric radiation.

It is, however, more satisfactory to study the effect of light in producing electromotive variation. Becquerel, Minchin, and others have shown that light produces electromotive variation in a photo-electric cell. Like electric radiation, the effect of light is not confined to any particular metal or groups of metals, but all metals exhibit an electromotive variation under its action. Two opposite effects are likewise shown; in some cases the potential is raised, in other cases the potential is lowered by the action of light.

I now proceed to show the remarkable similarity of the curves of effect produced by electric radiation and light. For the photo-electric cell I used two silver strips fastened at the back by paraffin on a glass plate. The front surfaces were exposed to bromine vapour. The two strips formed the two plates of the photo-electric cell, the electrolyte being common tap-water.

If the two strips are exactly similar, then there is no P.D. between them, and the effect of light on either of the strips is the same.

The two plates being opposed, there would be no resultant effect if both were illuminated.

But if the two plates are slightly different, then the effects on the two are not the same. There will then be an electromotive variation, even when both the plates are exposed.

9. *Effects of Flashes of Light.*

In fig. 9, (*a*), is shown the effect of flashes of light of 2 seconds duration on AgBr plate. The source of light—an incandescent gas-burner—was at a distance of 12 inches. If the plates are kept in the dark for several days, the sensitiveness is then very much enhanced, and

effects described below may be obtained by much feebler light, such as that given by a paraffin lamp or a candle. The strong similarity to

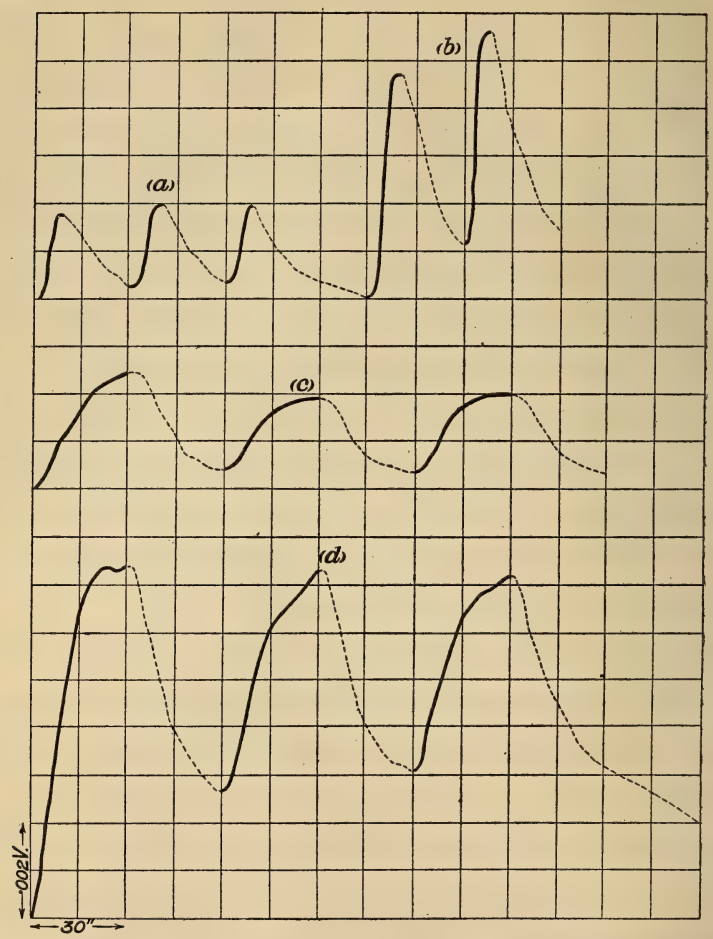


FIG. 9.—Electromotive variation in AgBr cell due to the action of light. Ordinate represents E.M. variation, and abscissa the time. Thick lines represent the effect of light and dotted lines the recovery. (*a*) and (*b*) represent the effect of flashes of light of 2 seconds duration. The distance of source of light in (*a*) = 12 inches; in (*b*) = 6 inches. (*c*) and (*d*) represent the effect of continuous light (duration 30″), distance of source of light in (*c*) = 18 inches in (*d*) = 9 inches.

the curve for single flashes of electric radiation (see fig. 4) will at once be noticed. The recovery curve is also convex to the abscissa. The time allowed for recovery was not sufficient to bring the substance

back to its original condition. The successive starting-points are therefore slightly ascending. In the last curve in the series (*a*) sufficiently long time was allowed, and the substance completely recovered in about 37 seconds. (*b*) shows the effect of light of about four times the intensity, the source being brought nearer to a distance of 6 inches. The effect is stronger, but not quite equal to four times the effect produced in the last case.

10. *Effect of continuous Action of Light.*

Just as in the case of electric radiation, light produces a maximum effect, corresponding to a given intensity. Fig. 9, (*c*), shows the effect of continuous light of 30 seconds duration, the source of light being at a distance of 18 inches. Observe the tendency of the curve to become horizontal when reaching the maximum. (*d*) shows the effect of four times the intensity, the light source being at a reduced distance of 9 inches. Here, too, the intensity of effect is not quite four times the effect of light at a distance of 18 inches.

11. *Reversal Effects.*

With electric radiation it was found that the effect of a flash was sometimes a transitory negative twitch immediately followed by the normal response. With light, too, it is frequently found, as has been observed by Minchin, that the immediate result is a transitory negative effect followed by the normal action. Under molecular modification there may sometimes be seen a reversed response.

If the action of light is continued, after the maximum effect is reached, there is produced a tendency towards, or actual reversal.

In fig. 10 is given an interesting series of results showing the growth of fatigue, the different phases culminating in actual reversal. (Compare with fig. 6.) It will be seen that in the second response was feebler, and

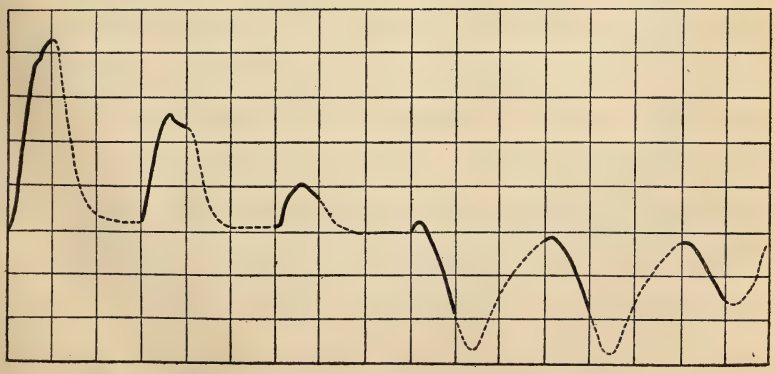


FIG. 10.—Fatigue and reversal in AgBr cell.

a tendency towards reversal had already taken place after an exposure of about 9 seconds. In the third, response is feebler still. In the fourth, during illumination, the normal response is extremely weak, and lasts for only 3 seconds; there is then a reversal in the response which is fairly strong. On the stoppage of light the effect continues for some time. Hitherto the recovery commenced immediately on the cessation of light. After the fourth, the responses are of opposite signs. They also get feebler and feebler.

It will thus be seen that both electric radiation and light produce similar conductivity and electromotive variations. In both, two opposite effects are observed. The curves of effect in both are similar. Under the action of continued radiation both exhibit a limiting effect. Under too long continued action, both exhibit a tendency towards or an actual reversal.

In the next paper I shall adduce further evidence tending to show that these effects are due to molecular strain.

"On the Similarities between Radiation and Mechanical Strains." By JAGADIS CHUNDER BOSE. Communicated by LORD RAYLEIGH, F.R.S. Received April 18,—Read June 20, 1901.

I have in the previous paper described the various interesting effects due to visible and electric radiation.

Different considerations were adduced which tended to show that these effects were due to molecular strain, produced in the substance by the action of radiation. The whole history of the change produced by radiation—both the direct and after-effects—was graphically recorded in the various curves given. It was supposed that the strain effect on a substance was attended with conductivity or electromotive variation. This supposition can be further verified by observing whether undoubted strain, which can be produced by mechanical means, does give rise to a conductivity or electromotive variation.

As regards the conductivity variation due to mechanical strain, it is well known that in the construction of standard resistance coils the effect of winding the wire on a spool is to produce a distinct variation of resistance, and that this strain effect can only be removed by annealing. The difference between the resistance of a substance when strained and after it is annealed is sometimes very considerable.

The effect of electric radiation is very great in changing the conductivity of a mass of discontinuous particles. It is to be borne in mind that the effect of electric radiation is only skin-deep. As the action is one of surface, the larger the surface the greater is the effect produced. We see that in loose particles the effective surface acces-

sible to radiation is very much enlarged. Moreover, the resistance offered to the particles is not due to the individual solid lumps, but to the resistance of *surface* layer. It is precisely the surface layers that are affected by radiation, and hence the marked variation of resistance.

When the particles become continuous, the radiation can only affect the extremely thin layer of molecules on the surface, the mass in the interior being shielded by the outer conducting sheet; the molecular changes produced on the surface layer do not affect to any appreciable extent the conductivity of the mass.

For detection of strain effect in continuous solids the method of electromotive variation is more suitable. We have seen that light causes a P.D. between the acted and unacted plate. We shall employ this method to find out whether mechanical disturbance gives rise to a similar electromotive variation between the acted and unacted plate.

1. *The Strain Cell.*

For the purpose of the experiment, I made a voltaic element composed of two pieces of the same metal wire, W.W′, cut from the same length. These are fixed parallel to each other in an L-shaped piece of ebonite, see fig. 11. The upper ends of the wires are in connection with the electrodes EE′, which lead to a very sensitive dead-beat galvanometer of D'Arsonval type. The wires at their lower ends are fixed to the ebonite piece by means of ebonite screws SS′. The upper ends, as has been said, are fixed to metallic rods EE′ (which also serve as the electrodes), kept moderately stretched by springs CC′. A long handle, A, provided with a pointer, could be attached either to E or E′, and by its means either of the wires could be twisted. The angle of the twist is measured with the help of a graduated circle, not shown in the figure.

If a cell be made of two clean wires cut from the same piece, with water as the electrolyte, there should theoretically be no P.D. between the two. But in practice there is found a small P.D. between the wires, owing to small difference in the molecular condition.

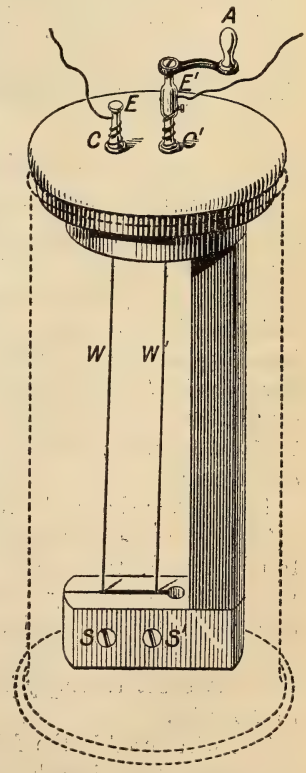


Fig. 11.—The Strain Cell.

But this initial difference can be annulled by appropriate means. One method is to stretch one of the two wires by which the initial difference may be neutralised. The particular wire which should be stretched is found by trial. When the cell is first made, there is some irregularity in the action owing to molecular instability. This can be made to disappear, if both the wires are vibrated for a time. After these precautions are taken, results are obtained which are extraordinarily consistent.

Now if one of the two wires be continuously twisted, an increasing P.D. is induced during torsion between the acted and the unacted. This may be measured by the deflection of a sensitive galvanometer. Curves could thus be obtained with electromotive force, measured by the galvanometer deflection, as ordinate, and the time during which disturbance is kept up as abscissa. Such curves were directly obtained by the recording apparatus described in my previous paper. The wire was twisted at a uniform rate. The successive dots represent the completion of 360°. To keep the deflection within the scale, a megohm was interposed in the circuit. The resistance of the cell was about 5000 ohms. The absolute values of electromotive force corresponding to the galvanometer deflections were subsequently obtained by noting the effect of a known electromotive force.

2. *Effect of Torsional Disturbance.*

Most of the metals—exceptions presently to be described—become negative during molecular disturbance caused by torsion, *i.e.*, the current through the *liquid* is from the acted to the unacted wire. As there is a considerable vagueness in the terms *positive* and *negative*, which has led to much confusion, I would name the acted wire as becoming *zincoid* or Z, when under an external disturbance the current flows *through the electrolyte from the acted to the unacted wire.* Again, in certain cases the reverse is true; the currrent flows from the unacted to the acted wire; the acted wire will in that case be designated as *cuproid* or C.

In fig. 12 is shown the effect of twist on Zn.

The induced electromotive variation is not due to twist as such, but to molecular disturbance induced during increasing twist. For if the wire be held stationary in the twisted position, the molecular disturbance with the attendant electromotive variation will gradually disappear. (Fig. 12.) Other evidences will be brought forward to show that the effect is simply due to the molecular disturbance, and not to the twist.

The wires used in the following experiments were from commercial specimens. The length was in every case about 9 cm., but the diameters were not the same.

If the conductivity variation under the stimulus of electric radiation and the electromotive variation under mechanical stimulus are but expressions of some molecular effect, we may expect the peculiarity of one kind of response reflected in the other. We shall presently see how closely the normal effects in the two classes resemble each other. Still more extraordinary are the similarities that exist even in abnormalities, several instances of which will be given later. I shall men-

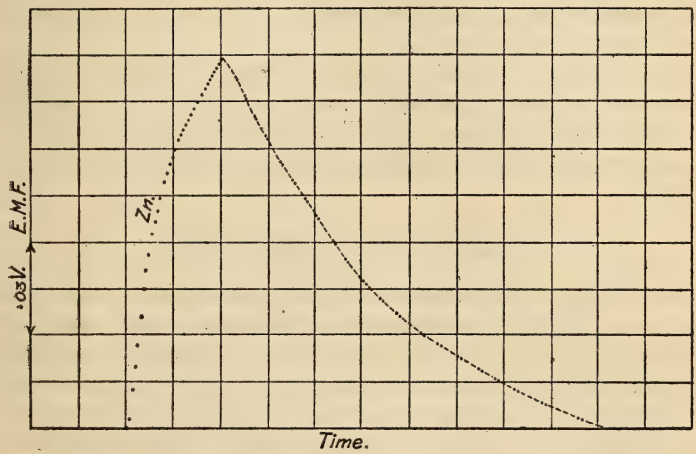


FIG. 12.—E.M. variation due to torsion of zinc wire. Successive dots in the ascending portion of the curve represent effect of rotation through 360°. The descending curve represents recovery.

tion here only one case. We have seen in experiments with electric radiation that substances sometimes fall into a sluggish molecular condition, when the responses almost disappear. Strong stimulation (induction shocks, &c.) or annealing is found to restore the sensitiveness. The same peculiarity is observed in the strain-cell. Lead, for example, specially on cold days, is apt to fall into a sluggish condition, when it becomes almost irresponsive. But it regains its sensitiveness after intense vibration or annealing.

All metals (including the noble metal Pt) when molecularly disturbed exhibit electromotive effect. The intensity of electromotive variation depends on the nature and physical condition of the substance. The intensity of effect does not, however, depend on the chemical activity of the substance, for the electromotive variation in the relatively inactive tin is greater than that of zinc. The electrolyte used in the following experiments is common tap-water, but similar effects are also obtained with distilled water.

3. *Self-recovery.*

It was said that the acted wire, usually speaking, becomes zincoid. This is not universally the case; there are substances which become cuproid under mechanical stimulus. I have previously said that electric radiation produces opposite effects on different substances; silver is often found to show an effect (increase of resistance) opposite to the generality of metals. It is very curious that silver is also often found to exhibit an opposite electromotive effect under twist, that is to say, the acted wire becomes C.

As long as the wire is not overstrained there is always a recovery. Observe the extremely regular recovery in the curve for Zn when the twisting was stopped. It will be noticed that the recovery is very rapid at first, but slow in the later part, and that the recovery is complete.

4. *Irreversible Molecular Effect of Twist.*

In the case of electric radiation or light, the impulses are of a vibrational nature, unlike the one-directioned mechanical twist used in the above experiments. To make the two sets of phenomena comparable, we should have the mechanical disturbance of a vibrational nature also. I therefore next tried to see what the effect would be of reversing the direction of the twist, and found that the induced electromotive force is independent of the direction of twist.

I next tried the effect of a complete torsional vibration. I twisted the wire suddenly through + 90°, then back to zero, then to − 90°, and again back to zero, the complete vibration being executed in half a second. It will be seen that under these conditions we have a mere vibration and no resultant twist. This gave rise to an electromotive variation, the magnitude of which simply depended, as will be shown later, on the amplitude of vibration. It did not matter in the least whether the vibration commenced with a right- or left-handed twist.

It may be stated here that similar electromotive variation is obtained by molecular disturbance produced by a tap.

I shall now describe the effect of mechanical stimulus of varying intensities and durations. The intensity may be varied by varying the amplitude of vibration. We shall also study the effect of a single stimulus, or the summated effect of rapidly succeeding stimuli.

A set of experiments on the effect of mechanical stimulus may thus be carried out parallel to those on the effect of radiation stimulus. It would then be instructive to compare the response-curves of mechanical and with those of radiation stimulus.

5. *Effect of a Single Stimulus.*

For studying the effect of mechanical stimulus, a voltaic element made of "tin" wire* is very suitable. Normal responses are easily obtained after annealing. As has been said before, any other metal may be used; I have, in fact, obtained as good results with platinum. But the advantage of tin is that the electromotive variation is comparatively strong; under favourable conditions the electromotive variation obtained is as high as 0·4 volt; another advantage is that it shows very little fatigue. On freshly making the cell, signs may be exhibited of abnormal irritability; this is due to the fact that a stable molecular condition has not yet been reached; but a more settled state soon supervenes, and after that a succession of responses is obtained which are extraordinarily regular and consistent amongst themselves.

That the responses are due to molecular disturbance in the acted wire may be shown by the following experiment. The wire is clamped below; when the wire is subjected to torsional vibration, there is produced a strong molecular disturbance with the attendant electromotive variation. If the wire is now released from the clamp and vibration imparted as before, there would be no electromotive effect.

In fig. 13 is given a series of curves for different "intensities" of vibration. For want of space I have given a few only of each series. As a matter of fact, the succeeding series would have been mere repetitions of those which preceded. I have taken as many as 500 successive records, and each record is a mere duplicate of the rest. The substance does not exhibit any appreciable fatigue, especially if a period be allowed for complete recovery. It will be seen that the rise is quick, whereas the fall is comparatively slow, specially in the later part.

If sufficient time be allowed the recovery is complete. (In the curves given only 30 seconds were allowed, hence the recovery was not complete.) On the cessation of disturbance the electromotive variation gradually disappears, the wires returning to their original condition, after which similar cycles of operation may be repeated for any length of time.

In the case of molecular distortion due to electric radiation we met with similar instances of complete recovery. It was then shown that the effect was not primarily due to any chemical action, but was due to physical strain, the recovery taking place as it were by the release of strained molecular springs.

With strain cells, there is no permanent change; the stimulated wire returns exactly to its original condition on the cessation of disturbance.

In the borderland between physics and chemistry no sharp line of

* By tin wire is meant what is sold as such, and used as electric fuse. It is a pliable alloy of tin and lead.

demarcation can be drawn between the two; such divisions are somewhat arbitrary. In the case of tin cell we have the two wires originally alike. When one wire is vibrated a difference of potential is observed between the strained and unstrained wires, the P.D. dis-

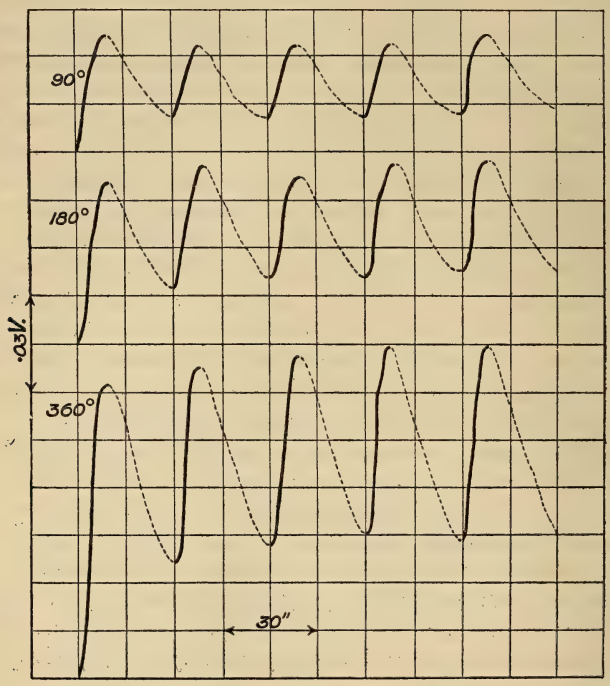


FIG. 13.—E.M. variation due to a single vibration through 90°, 180°, and 360° in a Tin cell. Period of vibration, 5″. Thick lines represent effect of stimulus, dotted lines represent the recovery.

appearing when the acted wire recovers from the strain. We may describe the same fact in chemical language by saying that owing to strain there is a tendency of the strained wire to become chemically more active (zinc-like), such tendency lasting only as long as it takes the wire to recover from the strained condition.

6. *Increased Effect with Increasing Intensity of Vibration.*

In fig. 13 are given the curves of response for single vibration, having amplitudes of 90°, 180°, and 360°, the period being 0·5 second. It will be noticed that the intensity of response increases with the energy of vibration.

7. *Effect of Summation of Stimuli.*

In the case of effect of rapidly succeeding flashes of electric radiation on Ag′, it was shown (see fig. 14) that the partial effects were fused together and there was produced a limiting effect, kept balanced by the force of restitution. With rapidly succeeding mechanical stimuli, we again obtain an exactly similar result. Fig. 15 (*a*, *b*) shows the effect of continuous vibration on tin cell, with different intensities of vibration, the vibration-frequency being two in a second. The curve gradually rises and attains a maximum, at which position it is

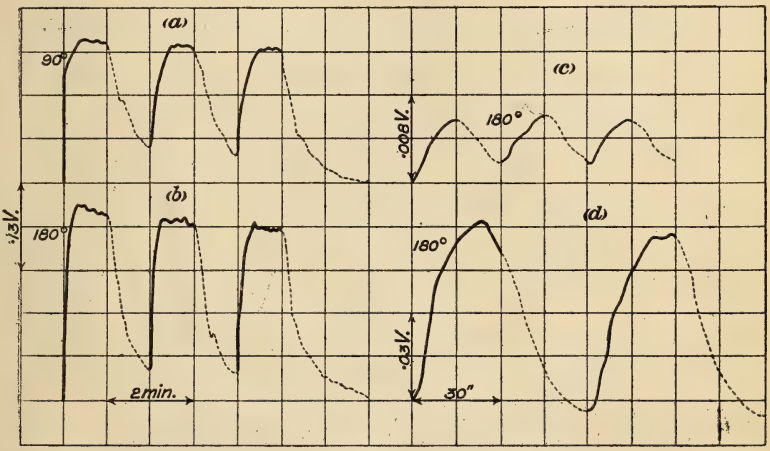


FIG. 14.—Effect of continuous vibration. (*a*) and (*b*) show effects on a Tin cell. In (*c*) the effect on the particular Silver cell; the sign of E.M. variation is opposite to that of Tin cell. (*d*) shows the effect on a Nickel cell.

held almost rigid as long as the disturbance is kept up. But on the stoppage of vibration there is an immediate recovery, and if sufficient time be allowed the recovery is complete, as seen in the last curve of the series. The disturbance was kept up for 1 minute, and the period of recovery allowed was also 1 minute. In this way I obtained a long-continued series of exactly similar curves, there being little fatigue; this is the case when a period of repose intervenes. But if the vibration is kept up without intermission signs of fatigue begin to appear, and the curve tends to fall. In some metals there may even be a reversal. Observe the flat top of the curve similar to that of Ag′ under electric stimulus mentioned above. Also the effects of different intensities of vibration, as shown in (*a*) and (*b*).

In (*d*) is shown the effect of vibration on Ni. After reaching the maximum there is a tendency towards reversal. Ni also shows greater signs of fatigue.

In (*c*), fig. 14, is shown the interesting curve for a given piece of Ag. The effect is very much feebler, and curiously enough it gave an opposite response, the vibrated wire becoming cuproid. It was said that silver occupied a peculiar position as regards response to electric radia-

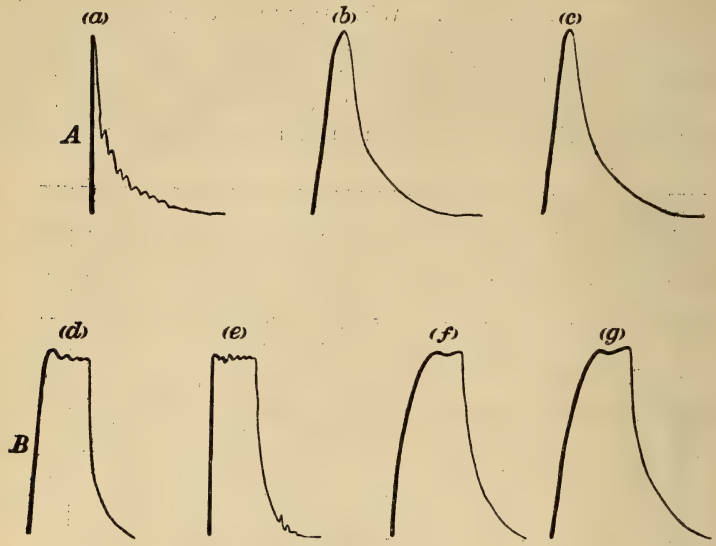


FIG. 15.

A. Effect of stimulus of short duration.
  (*a*) Effect of electric radiation on Ag′ (conductivity variation).
  (*b*) „ light on HgBr (E.M. variation).
  (*c*) „ mechanical vibration on Tin (E.M. variation).
B. Effect of continued action of stimulus.
  (*d*) Effect of continued action of electric radiation on Ag′ (conductivity variation).
  (*e*) „ „ „ mechanical vibration on Tin (E.M. variation).
  (*f*) „ „ „ light on AgBr (E.M. variation).
  (*g*) „ „ „ mechanical vibration on Nickel (E.M. variation).

tion, sometimes responding in one, and again in an opposite manner, probably owing to its readiness to pass from one molecular condition to another, under slightly different external conditions. With mechanical vibration, too, I find silver exhibiting opposite electric variation, the acted wire becoming on different occasions either Z or C.

8. *Reversal Effects.*

Reversed Effect due to Sub-normal Stimulus.—Just like the negative effect (*i.e.*, opposite to the normal) often exhibited under electric radiation

when the stimulus is below the critical intensity, so also it is found that a feeble mechanical stimulus often produces an effect opposite to the normal. Thus with strain cell made of lead, I found that whereas the acted wire became cuproid with an amplitude of vibration of 4°, the same wire when vibrated through 45° became zincoid. Thus in a Pb cell (50,000 ohms in circuit)

Amplitude of vibration.	Deflection.	Result.
4°	5 divisions to right	Acted wire C.
45°	70 ,, left	,, Z.

This effect I have often noticed. It was too frequent to be accidental, but it did not occur invariably. On the occasions when it happened, this negative effect disappeared after continued vibration. Thus on taking a record of effect of continued vibration, there is produced a negative twitch, which is converted later into a positive deflection, just as in the curves of effect of light in fig. 17.

Reversal produced by Continued Stimulation.—After the maximum effect is reached, if the vibration is still continued, there is a tendency for the curve to descend to the neutral line. In the case of nickel I have even found the curve reversed, that is to say, there was a complete reversal of electromotive force.

I have described the various molecular effects produced by mechanical stimulus under varying conditions, and shown how very similar in every detail they are to the effects produced by electric radiation and light. How striking these similarities are, will be seen from the following tabular statement and comparison of different curves.

9. *Molecular Effects common to Electric Radiation, Light, and Mechanical Vibration.*

1. The molecular effect produced may be detected either by conductivity or electromotive variation methods.

2. Substances when not overstrained exhibit recovery, but the recovery is delayed when there is overstrain.

3. Response modified by previous history, and the influence of the surrounding conditions. Slight rise of temperature and annealing generally favourable to increased sensitiveness and quick recovery.

4. Under the action of electric radiation, light, and mechanical vibration, two opposite effects are exhibited; by the conductivity variation method this is seen in the diminution or increase of resistance; by the electromotive variation method we get positive or negative variation.

5. In the curve of response, in all the above cases, the ascending portion is abrupt, whereas the fall during recovery is at first rapid,

then comparatively slow, the curve of recovery being thus convex to the abscissa.

6. Under rapidly succeeding stimuli, there is a fusion of individual effects; the curve rises to a maximum, when the force of restitution is kept balanced by the distorting force.

7. Sub-normal stimulus often produces a reverse effect. Too long-continued disturbance produces, or tends to produce, a reversal.

8. Under peculiar moleculiar modification, the response is of opposite sign to the normal. Continued stimulation converts abnormal to normal. The response curve may thus exhibit, at the beginning, a negative twitch followed by the normal positive.

A few curves are selected from experiments already described, and given below, in order to illustrate graphically the remarkable similarities of response to different kinds of stimulus. (See fig. 15.)

They show how essentially similar are the molecular effects produced, though the modes of stimulation and the methods used for the detection of effects produced are so different.

10. *Effect of Light Vibration balanced by Mechanical Vibration.*

I have hitherto spoken of the similarities of the radiation and mechanical strains, but have not yet said anything about their mutual relation.

It is known that in cases where electric radiation produces an increase of conductivity, mechanical vibration produces an opposite effect, *i.e.*, an increase of resistance. It thus appeared that we have here an exhibition of two opposite molecular effects. No definite conclusion could be drawn from this, however, as the increase of resistance may have been due to the mechanical separation of the conducting particles.

I then thought of trying the effects of light and mechanical vibration in producing electromotive variation in a strain cell. For this purpose I took a tin cell, and subjected one of the wires to the action of light and mechanical vibration alternately. The upper curve of fig. 16

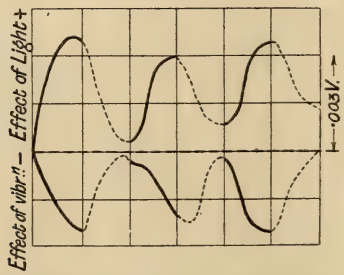


FIG. 16.—Effect of light and torsional vibration on a Tin cell. Light makes the acted wire cuproid, torsional vibration makes it zincoid.

shows the effect of light of a given intensity. It will be noticed that light makes the acted wire cuproid. But the action of mechanical vibration (see lower curve in same figure) makes the acted wire zincoid, and after several trials I found that a vibration with an amplitude of 3° produced a series of curves similar, but of opposite sign, to those produced by light. Thus mechanical vibration produced a molecular effect opposite to that of light.

I next allowed both the disturbing influences to act simultaneously on one of the wires, and the light action was then found to be exactly balanced by the action of mechanical vibration, an increase or diminution of either at once upsetting the balance.

The molecular effect of mechanical vibration thus appears, at least in the case of tin, to be opposite to that produced by light. This may be the case in general: the exception might be when one of the two stimuli is normal and the other sub-normal.

"On the Strain Theory of Photographic Action." By JAGADIS CHUNDER BOSE. Communicated by LORD RAYLEIGH, F.R.S. Received April 18,—Read June 20, 1901.

Our uncertainty with regard to the correct theory of photographic action is due to great experimental difficulties in studying the problem. As for instance:—

(1.) There is reason to believe that every substance is molecularly affected by radiation, but detection of change is rendered impossible by the imperfections of methods hitherto available, and also by the subsequent self-recovery of the substance in darkness. The effects can be detected in a few cases only when the changes produced happen to be visible, or become visible on development.

(2.) As regards direct chemical tests, if we take, for example, the case of AgCl, the quantity of radiation product is exceedingly small, and occurs in the presence of a very large amount of unchanged chloride. The isolation of the minute traces of changed product has baffled all effort. Again, there are produced various secondary reactions which complicate the phenomenon.

To arrive at a correct idea of the changes produced, it is necessary to measure the minute effects produced by radiation on the extremely thin layer—perhaps only a few molecules deep—of the sensitive substance. In order to ascertain this, it is desirable to begin with the study of some elementary substance in which its effects are attended with few secondary complications. And, lastly, it is necessary to have some means of studying all the stages of change in a continuous

manner, so that the important preliminary phase of "molecular negotiation" may not be missed. I have in my two previous papers shown how the above ideal requirements may be realised by taking advantage of the conductivity or electromotive variation methods.

These methods not only enable us to detect extremely minute molecular changes produced by radiation, but also to follow the changes moment after moment in a continuous manner.

I have described in the two previous papers the various molecular effects produced by light, electric radiation, and mechanical disturbance under different conditions. The consideration of these will give a clear insight into various obscure phenomena connected with photographic action, among which may be mentioned the following:—

1. Photo-chemical induction.
2. Relapse of invisible image.
3. Recurrent reversals.
4. The development of pressure marks.

1. "*Chemical*" *and* "*Physical*" *Theories of Photographic Action.*

It is an arbitrary distinction to call a phenomenon either physical or chemical when it happens to be on the common borderland. I have shown that when a substance is molecularly strained by light, its chemical activity is modified in consequence of the physical strain. The acted and the unacted portions will therefore be unequally attacked by a developer. In the case of a compound, the strain produced by light may cause a modification which renders it susceptible to decomposition by the action of a reducing agent. The observed évolution of chlorine when moist AgCl is exposed to the long-continued action of intense light is often adduced in support of the chemical nature of photographic action. This extreme case of dissociation cannot, however, be regarded as representative of the action of light in the formation of latent images. In ordinary photographic action we have merely the effect of a moderate stress producing the corresponding strain (with concomitant variation of chemical activity), and not the disruptive effect of a breaking stress.

With reference to photographic action, various facts are known which cannot be well explained from purely chemical considerations. In connection with this the following experiment of Professor Dewar is suggestive. It is found that at the low temperature of $-180°$ C., there is a cessation of all chemical action. Even such an extremely active substance as K does not show any action when immersed in liquid oxygen.* Now at these extremely low temperatures, where the action of such an active substance as K is suspended, an Eastman film was still found fairly sensitive to photographic influence.

* Dewar. Friday Evening Discourse at Royal Institution, June 26, 1891.

In the above case, it is difficult to see how light could have produced any chemical action in the relatively inactive silver salt. It is more likely that the effect produced was of the nature of some physical strain. That light does produce molecular strain even at such low temperatures —a strain which may remain latent—is shown from Professor Dewar's experiments on phosphorescence. Ammon. Pt. Cyanide cooled to $-180°$ C. in liquid air absorbs light, but emits feeble radiation. But as the temperature is raised the stored-up light is emitted with very great intensity.

I now proceed to consider the photographic interpretations of the various molecular response curves taken under the action of radiation, as detailed in my previous papers.

2. *Substances may be Sensitive and yet give no Photographic Image.*

The photographic effect on a sensitive plate is usually demonstrated by appropriate development, long after the exposure. The after-effect of light on the sensitive substance may be fugitive or persistent. There are numerous gradations of this persistency of after-effect.

In order that the effect of light may be "developed," it is therefore necessary that the portions corresponding to the image should not in the meantime have recovered from the strain due to radiation; for otherwise there would be nothing to distinguish the light-impressed portions from the other portions not affected by light.

Though almost all substances are molecularly affected by radiation, yet there is a great difference in the permanence of after-effects. The recovery, as has been mentioned before, is very quick in some cases, whereas in others it may be protracted.

It is obvious that any method which attempts to develop the after-effect a long time after the exposure will not be successful in cases where there is quick self-recovery. It will only be successful where the strain effect is more or less permanent.

It is thus seen that it is quite possible for a substance to be sensitive to radiation, and yet seem to show no effect capable of photographic development, owing to rapid self-recovery.

3. *Relapse of the Invisible Image.*

The above considerations afford a simple explanation of the very obscure phenomenon of the relapse of the invisible image. Recovery is merely a question of time. With certain substances it is immediate, with others it takes a little longer, as in a daguerreotype, where the latent image only disappears in the course of several hours. In ordinary photographic plates the recovery may not take place for several years. We have seen how the strain effect of electric radiation was transient in some cases, whereas it was persistent in others.

It is evident that in order to make the after-effect more or less permanent, and thus render it developable, self-recovery should be retarded. There are two ways in which the after-effect may be rendered comparatively permanent: (1) Even a highly elastic substance may be rendered more or less permanently distorted by straining it beyond the limit of elastic recovery; or (2) the presence of a retarding substance may prevent the self-recovery of the sensitive material. One of the chief functions of the so-called sensitisers may be to prevent self-recovery and make the after-effect permanent.

4. *Permanence of the Affect-effect by Overstrain.*

Thus in many cases where images cannot be obtained with ordinary exposure, they can be obtained with excessive strain caused by prolonged exposure. Thus Moser obtained an invisible image on a clean silver plate by exposing it to the sun for 2 hours or more. The invisible image was afterwards fixed by development with mercury vapour. A similar result was obtained with copper.

Major-General Waterhouse describes a very interesting series of investigations* in which by prolonging the exposure, printing-out impressions were obtained on silver. These could be developed not only by mercury or water vapour, but also by ferrous sulphate or pyrogallic developers. Images were also obtained on lead and gold.

All these results derive an additional interest from the fact that most of the phenomena that occur by the exposure of ordinary photographic plates containing haloid compounds of silver can also be observed upon a silver plate exposed to light. In my experiments on molecular effects produced by electric waves, I found *all* metals sensitive to electric radiation, owing to the extremely delicate nature of the conductivity method of detection. The molecular effects of visible radiation on various substances are also exhibited by the electromotive variation method. In the experiments of Waterhouse, a considerable number of metals were found to be sensitive to visible radiation, the effect being rendered more or less permanent by overstrain.

5. *Electromotive Variation Curve due to Light.*

I give below one out of several similar curves, showing the effect of continuous light on one of the two plates in a photo-electric cell of AgBr (see fig. 17). In this curve several distinct stages are noticeable.

(1.) A short latent period, where there is apparently little or no action or even a transitory negative action. The curve given had to be contracted to put in all the different phrases, and the peculiarities of the first part cannot be very well shown.

* Waterhouse, 'Roy. Soc. Proc.,' April, 1900.

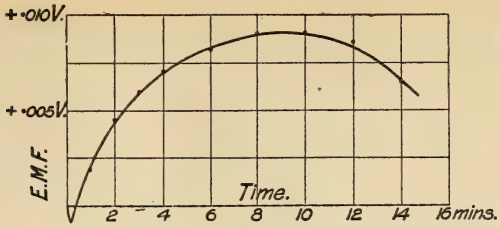


FIG. 17.—E.M. variation curve for AgBr cell, under the continued action of light. Note the preliminary negative twitch.

I have previously remarked that the molecular strain curve in general is in the first part slightly convex, then straight, and in the last part concave; this is true not only when the strain is produced by light, but also by mechanical vibration.

(2.) After this stage, the curve of response rises almost in a straight line. This is the stage of increasing action.

(3.) The curve then reaches the maximum and becomes horizontal; after which it begins to fall, till it reaches the original neutral line.

(4.) After very prolonged exposure I have sometimes found the curve proceeding in the *negative* direction, thus exhibiting molecular reversal.

I have before explained the similarities of the molecular strains produced by light and mechanical vibration. The recurrent reversals are also sometimes obtained with mechanical vibration, as in the following electromotive variation curve for nickel (see fig. 18), which was kept for a long time under constant mechanical vibration.

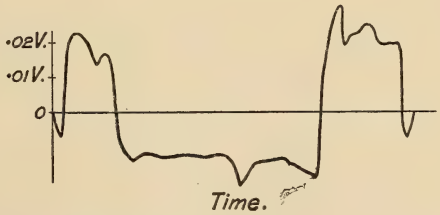


FIG. 18.—Recurrent reversals obtained with a Ni cell under continued vibration.

6. *Photo-chemical Induction.*

The first part of the curve, or the latent period, is very suggestive as regards the obscure phenomenon of photo-electric induction. Thus "Quantitative measurements have shown that the action of light is not instantaneous. On the contrary, it gradually develops, and requires a considerable time before it attains its full strength. When a mixture of chlorine and hydrogen, which have been kept in the dark, is exposed

to the light, there is either no hydrochloric acid or only a very small quantity formed in the first moment; but the rate of formation increases so that the quantity formed in a given time, *e.g.*, a minute, continues to increase until it attains a maximum value. Bunsen terms the gradual increase in the action *induction*. If the gaseous mixture has been once exposed to the light, it will retain in the dark, for about half an hour, its capacity for forming HCl in the light. If the gas has remained in the dark for a short period and is again brought into the light, it requires a very short period of induction; but the period of induction will be lengthened by keeping the mixture in the dark for a long time. [This is evidently due to self-recovery.—J. C. B.] Exposure to the light renders the gaseous mixture capable of entering into combination, but it does not bring about combination itself."*

The latent period of the curve, due to molecular inertia, would thus appear to offer an explanation of induction. In connection with this it is interesting to note the well-known fact that a very slight preliminary exposure of the photographic plates considerably enhances their sensitiveness.

It would also appear from the inspection of the curve, that the general law of photo-chemical action, which regards the total action as proportional to the product of the light intensity multiplied by the time of exposure, is subject to several modifying conditions. During the latent period, this cannot hold good in the first part, nor can it be true after the maximum is reached. It can hold good only in the second stage when the action proceeds uniformly.

7. *The Effect of Intermittence in Modifying the Law of Photo-chemical Action.*

But even after the substance has arrived at the second stage of uniform action, there may still be deviation from the above law. If in one case light be intermittent, and in the other continuous, the effects may be quite different, though the total durations be equal. For in the former case, during the continuation of light we may have distortion or molecular swing proceeding in a given direction, but on the stoppage of light, the swing stops too, sooner or later (sooner if the distortion has been considerable, when the force of restitution becomes great), and owing to self-recovery may even become reversed. After an interval, when the light is again allowed to act, it has not only to overcome the molecular inertia, but may have in certain cases to reverse the negative swing. In the case of continuous radiation, on the other hand, the molecular action proceeds without hindrance.

This is very well seen in the curve given below, which shows the difference in the extent of molecular effects produced in an AgBr

* Meyer, 'Modern Theories of Chemistry,' p. 507.

cell by interrupted and continuous illuminations of the same total duration. (See fig. 19.) Though light acted for the same length of time in both cases, yet in that of interrupted illumination the molecular effect as measured by the galvanometer deflection was only seven divisions, whereas with continuous illumination the deflection was 11·5, or one and a-half times as great.

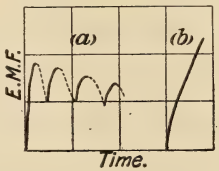


FIG. 19.—Effect of (*a*) intermittent and (*b*) continuous illumination. In (*a*) there are four interrupted illuminations of 15″ each, the total duration being 1′. In (*b*) there was continuous illumination for 1′.

It is thus seen that owing to self-recovery, the effect of light with intermittent illumination is less. It is also evident that the greater the period allowed for self-recovery (during the interval of darkness) the less will be the resultant effect. In connection with this, the experiments of Abney are very interesting. In experimenting on the difference between the effects on photographic emulsion of a continuous exposure and a series of intermittent illuminations, he finds that in the latter case the effect produced was always less, and that the longer the interval between the exposures, the smaller was the effect.

8. *Photographic Effect Modified by Time-rate.*

It will thus be seen that the photographic effect is not solely governed by the total amount of radiation, but by the time-rate also. The influence of this factor appears to be exhibited in the three following cases. Cases (2) and (3) derive an additional interest from the fact that the effects are probably due not to absorption of radiation, as is usually the case in photography, but to the emission of radiation.

(1.) In photographs of lightning, the line of discharge often comes out dark (the so-called dark lightning). It has been shown that reversals are produced by intense radiation; we may thus have reversals of the first, second, and succeeding higher orders. Now it is possible that the reversal, or the dark-lightning effect, may be obtained, not only by a subsequent diffuse illumination (Clayden effect), but also by the action of lightning itself, provided that the intensity of illumination is sufficiently great and sudden to produce the reversal. The luminous intensity of lightning discharge is incom-

parably higher than any that can be produced by a spark from an electric machine. Mr. R. W. Wood* obtained reversal with a single spark, when the photographic lens was wide open, but there was no reversal with four sparks, the lens aperture being reduced to one-fourth. The quantity of light was the same in the two cases, but the time-rate of illumination was different. This curious result would no longer appear anomalous, if we bear in mind the experiment in which the influence of time-rate was shown.

(2.) In trying to obtain photographs by heat radiation on sensitised papers coated with a mixture of silver and mercury iodides, the following curious effect was observed. The sensitised paper was exposed to heat radiation and became uniformly reddish in colour. A mask with cut-out letters was now put on it, and the sensitised paper was allowed to cool. The rate of cooling was very rapid at the places exposed by the cut-out letters, whereas at the covered portions the rate of cooling was very much less. After a long time when the sensitive paper had cooled down to a uniform temperature, prints were still visible, the effect being evidently due to the different rates of emission in the screened and unscreened parts.

(3.) Major-General Waterhouse in his paper† mentions an anomalous case which seems to be explicable from considerations given above. He took a polished silvered glass plate, and put it into a printing frame with a cut-out paper mask and mica screen in which were cut-out initials, just as if it were going to be exposed to the sun; but instead of exposure to light the plate was gently warmed for about 5 minutes over a spirit lamp, and then developed with mercury. The cut-out initials came out distinctly in dark lines. It seems to me that in this experiment, as the plate was uniformly warmed, the difference between the screened and unscreened portions could only be in the different rates of emission.

9. *Phenomenon of Recurrent Reversals.*

The fourth stage in the curve for the action of light (see fig. 16) will be found specially interesting with reference to photographic reversals. These reversals are found to be recurrent. Thus, starting with a neutral condition, we obtain the first negative with a moderate exposure; longer exposure will tend to reduce the intensity of the negative and give rise to a neutral condition. Further exposure gives rise to a *positive*, then a second neutral, and again a succeeding negative stage, and this often goes on in recurrent series.

Such recurrent reversals are also exhibited (see fig. 18) by a substance under continuous mechanical vibration. In my paper on "Elec-

* 'Nature,' November 30, 1899.

† *Loc. cit.*

tric Touch"* I have given similar instances of reversals produced by the action of long-continued electric radiation.

10. *Other Methods of obtaining Latent Image.*

If molecular strain be the basis of all photographic phenomena, then it ought to be possible to obtain latent images by other methods of producing molecular strain.

An instance of this is seen in the development of mechanical pressure marks. Images produced by electric strain are seen in the "inductoscripts."

11. *Conclusion.*

It is thus seen—

(1.) That molecular strain is produced by the action of light.

(2.) That as the physico-chemical properties of a substance are changed by strain, it is possible to develop the latent image through the difference in the following properties between the exposed and unexposed portions produced by light—

(*a*.) Difference in adhesive power, *e.g.*, development of daguerreotype by mercury vapour, development by water vapour.

(*b*.) Difference in chemical stability, *e.g.*, development by reducing agents.

(3.) That molecular strain may not only be produced by visible or invisible radiation, but also by (*a*) electric induction, (*b*) mechanical distortion. Latent images produced by such means may be developed, *e.g.*, inductoscripts, development of pressure marks.

(4.) That nearly all substances are sensitive to radiation, but the effect cannot in all cases be rendered visible, (*a*) owing to want of suitable chemical developers, (*b*) owing to quick self-recovery. The molecular effect due to radiation can, however, be demonstrated by the conductivity or electromotive variation methods.

(5.) That the latent period of overcoming inertia corresponds to the photographic induction period.

(6.) That the relapse of image is due to self-recovery.

(7.) That owing to the tendency towards self-recovery the radiation effect does not solely depend on the total quantity of light, but depends also on the time-rate of illumination. Hence the photographic effects of intermittent and continuous illuminations are not the same.

(8.) That the continuous action of radiation produces recurrent reversals.

(9.) That the molecular effects produced by light and electric radiation are similar.

* 'Roy. Soc. Proc.,' vol. 66.

"On the Interpretation of Photographic Records of the Response of Nerve obtained with the Capillary Electrometer." By GEORGE J. BURCH, M.A., F.R.S., Physiological Laboratory, Oxford. Received February 11,—Read February 20, 1902.

Preliminary Note.

Attention is specially directed to the following changes in the terms used to describe the electrical phenomena of living tissues :—

Old term.	New term.
Negative phase, or first phase...	Electro-positive phase, or first phase.
Positive phase, or second phase	Electro-negative phase, or second phase.
Galvanometrically negative.....	Positive, electro-positive.
Galvanometrically positive	Negative, electro-negative.

This terminology has been advocated for some time by Dr. Waller, who drew attention to it at the International Physiological Congress at Turin, 1901, and is now adopted by Professor Gotch and myself as being more in accordance with the phraseology employed by physicists in similar cases.

In Parts I and II, where the subject is treated from a purely physical standpoint, special terms have been used in order to avoid the confusion that might have arisen owing to the different meanings attached by physiologists to certain words used by physicists.

Thus—

Bundle	= Nerve or Muscle.
Linear conductor	= Nerve-fibre or Muscle-fibre.
Point of origin	= Exciting electrode, &c., or nerve-ending, &c.

In Part III the ordinary physiological terms are employed.

Statement of the Problem.

In previous papers on the capillary electrometer I have shown how it is possible, from the curves obtained by photographing on a rapidly moving plate the excursions of the end of the column of mercury, to draw derived curves representing the variations of the difference of potential by which these excursions were produced.

I have pointed out* that the photographic records obtained when two currents of definite potential difference and opposite in direction, lasting respectively as long as the first and the second phase of the electrical response of muscle, are thrown in succession into the electro-

* 'Phil. Trans.,' A, vol. 183, p. 100.

meter, differ in form from the records of the muscle response. In the former case the movement of the meniscus commences suddenly—the velocity is maximal at the commencement—the change of direction is sudden, and the end of the second phase is sudden also. It is quite otherwise with the records of the electrical response of muscle or nerve. There is great variety in them. The movement generally commences gradually. Maximum velocity may occur early or quite near the end of the first phase. The reversal of direction may be so sudden as to form a cusp, or the curve may be flat-topped for 0·001 sec. or 0·002 sec. The second or electro-negative phase may be cut short, or may exceed the first or electro-positive phase in magnitude. It may end almost abruptly or tail off so gently that it is difficult to determine when it ceases. But so long as the conditions are unaltered the same shaped curve is produced on repeating the experiment.

These varieties among the records are therefore due to characteristic peculiarities of the preparations.

But the characteristic peculiarities of a preparation may depend on the one hand upon its physiological state, and on the other upon purely physical and experimental conditions. It is necessary therefore to trace the influence of these latter on the form of the records, in order, by a process of elimination, to discover the results due to physiological differences.

I propose therefore in the present communication to show that it is possible to obtain further information by applying to the derived curves a process of interpretation based on purely physical grounds, and shall avoid dealing with the physiological side of the question except so far as may be necessary for the sake of clearness.

The first statement of the problem is best made from the experimental standpoint, and may be expressed briefly as follows* :—

* [*Note, added April* 10, 1902.—This is an expression from a purely physical standpoint of the well-known physiological theory of which the experimental basis is in brief:—

1. Du Bois Reymond's demonstration that the excitatory process in nerve is associated with electrical phenomena.

2. The classical experiment of Helmholtz, showing that the excitatory process in motor nerve, as judged by the time of the muscle response, is transmitted along the nerve at a definite rate.

3. Bernstein's proof, by means of his revolving rheotome, that the electrical phenomena are transmitted in the form of a wave at the same rate.

A great deal of work on the subject has been done with the revolving rheotome by Hermann, Bornttau, Hering, and others.

Professor Gotch and I have discontinued the use of the revolving rheotome, because, in the first place, the condition of the nerve is liable to be altered by the rapid series of excitations; in the second place, because the after-effect of each excitation is mixed up with those of succeeding ones; and in the third place, because the capillary elctrometer is far more sensitive.

It is this greater sensitiveness that has enabled me to push the investigation so much farther than has hitherto been attempted.]

A source of electromotive force is developed at a given point in a bundle of linear conductors imperfectly insulated from each other, is propagated* in both directions along the bundle, and finally subsides. It is required to investigate the variations of potential difference between any pair of points on the conductors.

Obviously the conditions are extremely complex, and a complete solution is impracticable because, from the nature of the case, it is impossible to determine the amount of the leakage from each conductor into the bundle. It is possible, however, to separate from the results those which depend upon purely physical conditions, and thus to clear the ground for the discussion of the truly physiological phenomena. In analysing photographs taken in this laboratory I have met with illustrations of all the points brought forward in this paper.

In thus dealing with the problem, it is necessary to take the following considerations into account:—

(1.) That the linear conductors (or briefly the conductors) constituting the bundle are not necessarily all of the same length.

(2.) That the electrical change originates in each conductor at a certain point of its length, from which it is propagated simultaneously in both directions.

(3.) That the points at which the electrical change originates in the different conductors may be all situated in the same cross-section of the bundle, or may be distributed along a certain portion or portions of its length.

(4.) That the development, as also the subsidence, of the E.M.F. at any given point of a conductor may conceivably be gradual or sudden, *i.e.*, the change from zero to maximum, or *vice versâ*, may be instantaneous, but is not necessarily so.

(5.) That the time relations and intensity of the electrical change may be temporarily or permanently modified at a given point in any or all the conductors of the bundle.

In stating the problem it is necessary to put it into such a form as will represent these conditions.

The simplest way of doing so is to deal first with the case of the single linear conductor, and to express the electrical changes in it as

* [*Note, added April* 10, 1902.—I can find no exact physical analogue to this phenomenon, and no other single word to express it than this physiological term.

It has been, I believe, compared to the lighting of a train of gunpowder in the middle. The flame spreads outwards in both directions, but dying out first in the middle, separates into two tracts of flame of practically constant length, travelling away from each other.]

the sum of two functions—one representing the development and the other the subsidence of a wave of electromotive force passing along it. Having thus dealt with the single linear conductor, the more complex case of a bundle of conductors may be expressed as the sum of a number of similar functions with different constants.

But although this method of proceeding is convenient for examining the experimental results, it only represents indirectly the facts which it is our object to investigate.

The real problem may be stated thus: Conceive three similar consecutive short portions, A, B, C, of a single nerve fibre, such that each of them may be regarded as a complete element of the fibre; it is required to determine—

1. The development, duration, and subsidence of the electrical changes in either of them.
2. Those conditions in B which enable it to become active under the influence of A.
3. Those conditions in B which enable it to induce a state of activity in C.

It will be observed that 1, 2, and 3 enter simultaneously into almost every possible experiment, but may be separately investigated by two distinct methods, namely, the physical—*e.g.*, alteration of position of leads, exciting electrodes, &c.—and the physiological—*e.g.*, influence of temperature, electrotonus, reagents, injury, &c. But whether the method be physical or physiological, the analysis of the photographic record merely gives the sum of the electrical changes occurring between two fixed electrodes at any given moment, so that in either case the analysis itself has to be interpreted in order to show how the curve is to be explained in terms of 1, 2, and 3.

It will be observed that no assumptions are made either as to the cause of the electrical phenomena, or the mode in which they are related to the activity of the tissue, beyond the fact that there is in muscle or nerve a potential gradient between a part in a state of physiological activity and neighbouring parts at rest. The E.M.F. might, so far as this investigation is concerned, be a function of the active condition, or of the transition from one state to another. In either case the method of dealing with the records would be the same. But having ascertained the meaning of the records, it may be possible by comparing them to determine whether the electrical changes are essential, or merely concomitant phenomena, of the active state.

I. *Variations of Potential Difference between Two Points of a Linear Conductor traversed by a Source of E.M.F.*

In the accompanying diagram (fig. 1) the successive stages of the electrical condition are represented graphically, and arranged in order of time. An electromotive force is supposed to originate at the point P, from which it spreads in both directions, the shaded portions indicating the parts in which it exists, and those not shaded the parts not yet reached by it, or no longer affected. The shaded parts may therefore be considered as positive to the unshaded. After a certain interval the effect subsides. This is represented as occurring first at the point from which it originated. Obviously, if the duration of the active period is the same for each point of the linear conductor, the time curve of the subsidence of the wave of E.M.F. will be parallel to its development. It does not, however, necessarily follow that it must be so, and hence the cessation of electrical activity may be more conveniently represented as a separate function of the time.

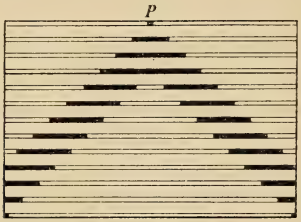


FIG. 1.—Diagram showing the successive positions of a wave of electromotive activity originating at the point P of a nerve-fibre, and travelling outwards in both directions. The shaded portions are positive to the unshaded.

The general effect of varying the rate at which the electrical change is propagated, and its duration in various parts of the linear conductor, is shown in figs. 2 to 7. They are drawn on the same plan as fig. 1, that is to say, time is reckoned from above downwards, and position on the linear conductor horizontally. The excitation is supposed to take effect at the centre of the conductor, and to spread symmetrically to the right and to the left. But in order to show more forcibly the relation between the above-mentioned two modes of expressing the problem, the right-hand half of each figure represents the electrical change as a wave passing along the conductor, while the left-hand half gives the commencement, duration, and end of the electrical activity at a series of points on the conductor.

The relation of the electrical changes to variations of potential difference between any given pair of leads may be studied by drawing a pair of vertical lines the required distance apart on tracing cloth,

and applying them to the diagram so as to represent by their position the position of the leads. The first phase begins when the wave of electrical activity reaches the first lead, and continues until it either leaves the first lead or arrives at the second. The second phase begins whenever the wave has both left the first lead and arrived at the second. But if neither or both the leads are affected by the wave, there is zero potential difference between them.

In fig. 1 the duration of the active period is constant for all parts of the conductor. This involves equality between the rate of propagation and the rate at which the disturbance dies out along the conductor. Both the length of the wave and the duration are constant.

In fig. 2 the velocity of propagation of the development is greater than that of the subsidence of the effect. Both the duration and the wave-length increase regularly. The converse of this is shown in fig. 3, where the wave of development travels slower than that of subsidence, corresponding to a regularly diminishing wave-length and also duration.

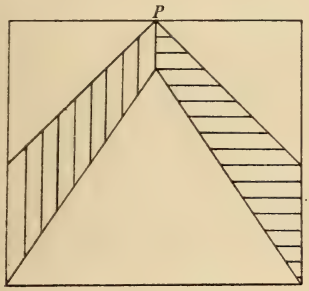


FIG. 2.—As in Fig. 1, but the duration of the active condition increases continually, as indicated by vertical lines on the left side. This corresponds to an increasing length of wave, as shown by the horizontal lines on the right.

N.B.—Time is measured vertically downwards, and position on the nerve-fibre horizontally.

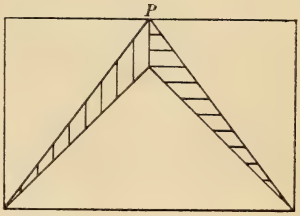


FIG. 3.—As in Fig. 2, but the duration continually diminishes (left side), corresponding to a continually diminishing wave-length (right side).

Manifestly under such conditions the response would not be propagated beyond a limited distance. I have some reason for believing that responses of this character might be obtained from kept muscle with minimal stimuli.

Up to this point there has been no marked difference between the two ways of representing the problem. With fig. 4 it is otherwise. Here the rate of propagation is constant, but the duration, as shown

on the left side, suddenly increases at a certain point from 3 to 5 units. Regarded as a wave of electrical activity, this *sudden* change is equivalent, as the right side of the figure shows, to a *gradual* increase of wave-length to a new constant value.

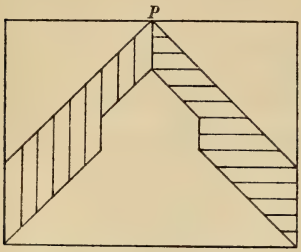


FIG. 4.—A sudden definite increase of duration (left side) implies a gradual transition from a constant short wave to a constant long wave (right side).

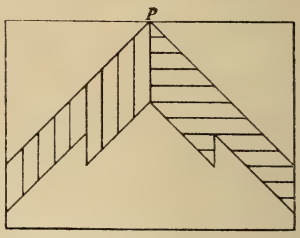


FIG. 5.—A sudden definite diminution of duration (left side) corresponds to a separation of the wave of activity for a short time into two portions (right side).

Fig. 5 is the converse of fig. 4, the duration suddenly diminishing at a certain point from 5 to 3. This diagram brings out the curious result that under these conditions the wave may split up into two portions. Moreover, for certain positions of the leads, the record in such a case would be monophasic.

Figs. 6 and 7 show the effects of a change in the rate of propagation, unaccompanied by any change in the duration. Two things should be noticed—first, that the slower rate of propagation corresponds to a

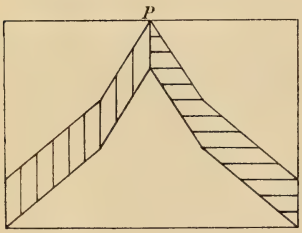


FIG. 6.—A sudden definite increase in velocity of propagation, the duration remaining constant (left side), involves a gradual increase of wave-length from one fixed value to another (right side).

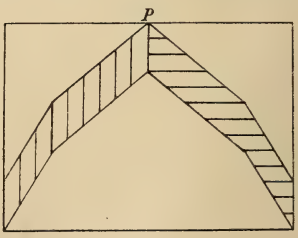


FIG. 7.—A sudden definite decrease in velocity of propagation, the duration remaining constant (left side), involves a gradual shortening of the wave from one fixed length to another.

shorter wave unless the duration is increased in the same proportion, and farther, that whereas the change of rate is sudden, the change of wave-length is gradual.

Problems of this type arise when two portions of the same nerve are kept at different temperatures. They may be worked out graphically, or by writing a separate formula for each portion where a sudden change occurs.

Proceeding to a closer examination of the problem, it will be convenient to consider in the first place the variations of P.D. due to the propagation of the wave-front of electrical activity. It may be assumed provisionally that its rate of propagation is constant, and that its passage is marked by a sudden definite rise of potential. As a farther simplification, the linear conductor is assumed to be of indefinite length, the origin of co-ordinates being situated to the left of the portion under consideration, so that the position of each of the leads connecting it with the electrometer may be represented by a positive quantity.

Let the line ON, fig. 8, represent a linear conductor of indefinite length, and let P be the point at which the electrical change originates.

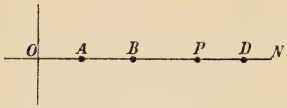


FIG. 8.—Needs no explanation, save the text.

Let the distance OP $= p$. Let leads connecting ON with an electrometer be placed at any two of the points A, B, or, D, and let their respective distances from O be a, b, d.

Let $T = t + s$, be the time that has elapsed since the instant of stimulation, s representing the *latent* period if any exists, and t being the time that has elapsed since the response commenced. In most cases s is eliminated, and therefore t will be used in the formulæ. The exceptions are dealt with on p. 211.

Let x represent the distance from O of the wave-front of the electrical change at the time t. Then $x = p \pm vt$, where v is the velocity of propagation of the wave-front in centimetres per second. The sign of v is positive for all points to the right of p and negative for all points to the left of it.

In the simple case under discussion, namely, that of a sudden definite rise of potential at the wave-front, it is manifest that a difference of potential will be established between A and B as soon as the wave-front has passed B, and that it will cease when the wave-front reaches A, both electrodes being then at the higher potential: *i.e.*, the P.D. begins when $b = p - vt_1$, and ends when $a = p - vt_2$.

Hence $$t_1 = (p-b)/v, \qquad t_2 = (p-a)/v;$$

and the duration of the difference of potential between A and B is

$$t_2 - t_1 = (b-a)/v.$$

Inasmuch as this expression does not contain p, it is evident that the duration of the P.D. between two leads both of which are situated on the same side of P is independent of the position of the point at which the electrical change originates, and is conditioned solely, so far as the wave-front is concerned, by the distance between the two leads. On the other hand, the period of delay between the moment at which an E.M.F. is first established at P, and the commencement of the P.D. between the leads, varies directly with the distance of P from B.

The case is otherwise when leads on both sides of P are taken, *e.g.*, B and D. The wave-front reaches B when $t_1 = (p - b)/v$, and it reaches D when $t_2 = (p - d)/-v$.

Suppose D is nearer to P than B is, this will make a P.D. in the same direction as in the previous example. Then $t_2 < t_1$, and the duration of the P.D. is,

$$t_1 - t_2 = \frac{p-b}{v} - \frac{d-p}{v} = \frac{2p-(b+d)}{v}.$$

Since by the diagram $d > p > b$, we may write $b + k = p$ and $p + l = d$. Then

$$t_1 - t_2 = (k - l)/v.$$

This expression is clearly a maximum when either $k = 0$ or $l = 0$. It will be observed that the sign of the resulting P.D. varies according as k or l is greater. Hence the duration of the P.D. will be greatest when one of the leads coincides with P, zero if P is midway between them, and it will vary in sign according as D or B is nearer to the point at which the E.M.F. originates. The delay will be zero if either electrode coincides with P, and cannot exceed the maximum of $(d - b)/2v$, at which point the duration becomes zero, and the value of the P.D. consequently vanishes.

The next step is to trace the time relations of the P.D. between the terminals which may result from the removal of the source of E.M.F. If the condition of electrical activity after having invaded the whole conductor begins at any moment to subside simultaneously at all points, and if the rate of subsidence is the same at all points of it, no P.D. will be produced between any two points, however situated. If the electrical activity begins to subside before it has invaded the entire conductor, and while the wave-front is still between the two electrometer leads, then if it subsides simultaneously over the whole part affected, the P.D. between the leads will simply fall to zero.

If the electrical activity, after having invaded the whole conductor, subsides first at the two ends of it, persisting longest at P, where it originated, the period of the subsidence will be marked by a second P.D. between the leads, of the same sign as that evoked by the wave-front of electrical change.

But by observation it is found that the first electrical change (in the case of muscle or nerve) is followed by a second in the opposite direction, and therefore we are justified in assuming that the subsidence of the condition of activity in the case of muscle and nerve takes place first at the point at which it originated.

We may therefore designate the position in the linear conductor of the wave-front of cessation from the active condition by an expression of the form,

$$x_1 = p \pm v_1(t+\theta),$$

where θ represents the duration of electrical activity in the portion of the linear conductor under P. If the central portions of the linear conductor remain active longer than those nearer the ends, then $v_1 > v$. If the period of activity becomes longer as the ends are approached we must have $v_1 < v$.

But if under normal conditions each portion of the linear conductor becomes active to the same degree, and for an equal period, then $v_1 = v$, and the progress of the wave of electrical activity must be represented by two parallel curves, one indicating its development and the other its subsidence for successive points along the linear conductor.

Hence in the normal case now under consideration we may put $v_1 = v$. Then, between the leads A and B, B becomes negative to A when the wave of cessation

$$x_1 = p - v(t-\theta) \text{ reaches B, } i.e., \text{ when}$$
$$t_1' = (p - b + v\theta)/v,$$

and remains negative to t until the wave-front reaches A, *i.e.*, when

$$t_2' = (p - a + v\theta)/v.$$

The duration of the P.D. is as before

$$t_2' - t_1' = (b - a)/v,$$

the two waves differing only by a time-constant θ.

Taking now the combined effect of both waves, it is evident that the form of the resulting curve of P.D. must depend upon the relation between θ and $(b - a)/v$.

If $(b - a)/v$ is less than θ then the first phase of the effect upon the leads A and B will be over before the second phase begins, and therefore there will be an interval of $\left(\theta - \frac{b-a}{v}\right)$ between them during which the P.D. will be zero.

If $(b - a)/v = \theta$ then the first phase will be succeeded by the second phase with no interval between.

If $(b - a)/v$ is greater than θ then the second phase starts before the first is over, but being of opposite sign, and *ex hypothesi* equal in

intensity, neutralises it and produces zero P.D. for a period equal to $\left(\frac{b-a}{v}-\theta\right)$.

Since the value of $(b-a)/v$ can be varied by shifting the leads A and B, it follows that θ can be determined by ascertaining at what distance between the leads the two phases of the P.D. follow one another without a zero interval between, for the sequence of potential differences will be

(1) With A and B close together,

$$0,\ +,\ 0,\ -,\ 0.$$

(2) With A and B at one particular distance,

$$0,\ +,\ -,\ 0.$$

(3) With A and B far apart,

$$0,\ +,\ 0, -,\ 0.$$

The case of leads on either side of P is similar, the phases of the resulting P.D. varying according to the relation

$$(k-l)/v > \theta;\ 0,\ \pm,\ 0,\ \mp,\ 0.$$
$$(k-l)/v = \theta;\ 0,\ \pm,\ \mp,\ 0.$$
$$(k-l)/v < \theta;\ 0,\ \pm,\ 0,\ \mp,\ 0.$$

II. *Variations of P.D. between Two Points of a Bundle of Linear Conductors traversed by a Source of Electromotive Force.*

Hitherto it has been assumed that the rise of P.D. at the wave-front of electrical activity is sudden, and its fall equally sudden at the end of the wave, and also that the electrical change is not complicated by the structure of the linear conductor in which it occurs.

It is necessary to investigate farther the modifications which may result when a number of such linear conductors act together.

In my paper on the "Time Relations of the Capillary Electrometer,"* I pointed out that in the case of muscle the rise of potential difference at the wave-front is not sudden, and the same is shown with respect to nerve in the analyses published by Professor Gotch and myself. Moreover, in the discharge of the electrical organ of Malapterurus, the E.M.F. is gradually developed, although in this case the phenomenon cannot be accounted for by propagation. There is sufficient evidence to warrant the introduction into the formula of a term expressing gradual development of E.M.F.

In dealing with the phenomena of a bundle of linear conductors, three things have therefore to be taken into account, viz. :—

* 'Phil. Trans.,' A, vol. 183, pp. 100, 104.

A. The points of origin of the wave of activity in the several linear conductors may be differently situated in the bundle.

B. The development of the E.M.F. at any given point of a linear conductor may be gradual, and so also may its subsidence, and the rate of subsidence may be different from the rate of development.

C. The constituent linear conductors may not all extend to both of the leads selected.

A. *Influence of the Position of the Points of Origin in a Bundle of Linear Conductors.*

Let the points of origin $P_1, P_2, P_3, \ldots P_n$ referred to O as origin of co-ordinates, be $p_1, p_2, p_3, \ldots p_n$.

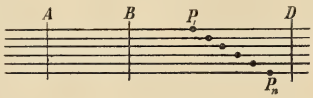


FIG. 9.—A bundle of linear conductors, connected with the electrometer at any two of the points A, B, D. $P_1 \ldots P_n$ are the points at which the electromotive change originates.

(1.) It is evident that with respect to the leads A and B, the duration of each phase of the effect will be alike for all the linear conductors, namely, $(b-a)/v$.

But the initial delay will differ for each, being $(p_1-b)/v$, $(p_2-b)/v$, &c., the amount of this difference being $(p_2-p_1)/v$, &c.

If all the conductors constituting the bundle were perfectly insulated from each other, since all the E.M.F.'s would be in parallel, there would be no higher P.D. produced by the joint action of any number of conductors. But if, as is probable, the short-circuiting is considerable, though of undeterminable amount, it may be assumed provisionally that the effective P.D. between the leads varies directly according to some function of the number of active conductors in the bundle. The problem then resolves itself into one of summation.

The "duration" of the effect, so far as the wave-front is concerned, is counted from the beginning of the earliest to the end of the latest effect. Hence the duration must be dependent partly on the distribution of the points of origin in the several linear conductors constituting the bundle.

Each conductor, as it flashes into activity, keeps up the P.D. for a time given by $t = (b-a)/v$, but its contribution arrives early or late according to the position of P.

We may therefore write,

$$t_1 = (p_1-b)/v\,; \qquad t_n = (p_n-a)/v\,;$$

and

$$t_n - t_1 = (b - a + p_n - p_1)/v,$$

i.e., the duration is increased by an amount equal to the time required by the wave to traverse the whole length occupied by the points of origin.

The rise of P.D. will be more or less gradual, and so will its subsidence, although the actual changes in each constituent conductor are still supposed to be sudden and definite in amount. The effect of a gradual development of E.M.F. will be dealt with later.

It is manifest that the most favourable conditions for studying the distribution of the points of origin are when the leads are far enough apart to separate the two phases of the response by a zero interval.

(2.) With leads B and D, *i.e.*, on both sides of a group of points of origin, the case is different (see fig. 9).

By the same formula as before,

$$t_1' = (d - p_1)/v; \qquad t_1^n = (d - p_n)/v;$$

$$t_2' = (b - p_1)/v; \qquad t_2^n = (b - p_n)/v;$$

and the duration is comprised between the smallest and the greatest value of t.

Now if $\overline{P_nD} < \overline{BP_1}$ then is $\overline{BP_n} > \overline{P_1D}$.

For let $\overline{P_nD} = \alpha$, $\overline{BP_1} - \overline{P_nD} = \beta$, $\overline{P_1 - P_n} = \gamma$.

Then $\overline{BP_n} = \alpha + \beta + \gamma$, which is greater than $\overline{P_1D} = \alpha + \gamma$.

Hence in this case the total duration of the first phase, due to the wave-front, is governed by P_n the point of origin nearest to either lead, and the direction or sign of the P.D. depends on which lead it is nearest to.

From this it follows that if any two similar conductors, P_1 and P_m, in the bundle have their points of origin of E.M.F. situated at equal distances on either side of the middle point between B and D, they will neutralise each other, not only as regards the P.D. resulting from the development of the wave of E.M.F. in them, but also as regards the effect produced by its subsidence.

Similarly P_2 will neutralise P_{m-1}, and P_3 will neutralise P_{m-2}, so that no difference of potential will result save from the portion $P_{m+1} \ldots P_n$, *i.e.*, the points of origin not symmetrically situated between the leads B and D.

This consideration indicates a method which I have occasionally employed of locating the mean position of a group of points of origin.

(3.) With points of origin partly between and partly beyond the leads, as in fig. 10.

Here $\overline{B.P_n}$ is the longest distance, and therefore governs the superior limit of duration. But D itself is over a point of origin,

hence $p_d - vt_1 = d$ marks the beginning of the difference of potential, and this gives $t_1 = 0$. It should be noted that, as in the previous case, if any two similar conductors have their points of origin of E.M.F. situated at equal distances on either side of the middle point between B and D they will neutralise each other, and thus reduce the total P.D.

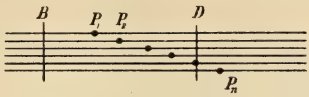


FIG. 10.—As in Fig. 9. B, D, electrometer leads. $P_1 \ldots P_n$, points of origin.

In fig. 11, p_1, from its position would add nothing to the P.D. until the wave-front starting from it had reached D on the one side. But its effect would cease the moment the opposite front of the wave reached B. The maximum would therefore be as intense as with all the points of origin outside the leads, but it would last a shorter time and appear to develop more slowly.

In this connection it may be observed that the form of the rise of potential difference between the leads B and A gives the distribution of the points of origin $p_1, p_2, p_3, \ldots$ in the bundle, whereas the form of the rise of potential difference for the leads C and D gives the distribution of p_n, p_{n-1}, p_{n-2}, &c.

Furthermore, it is evident that the curve of the second phase, in all cases where the leads are far enough apart, must be a repetition, reversed, of the curve of the first phase, so far as it is conditioned by the distribution in the bundle of the points of origin, and that any difference between the first and second phases must be due either to interference through overlapping or to some difference, general or local, in the time relations of the development and subsidence of the wave of electromotive force, or of its rate of propagation. The analyses show that such a difference exists.

B. *Influence of the Rate of Development and of Subsidence of the E.M.F. at a Given Point of each Conductor in the Bundle.*

The possibility that the development of the E.M.F. at successive points of a single linear conductor may not be sudden but gradual, must also be taken into consideration.

The effect as regards the time relations of the difference of potential at the leads is in the main similar to that produced by a bundle of conductors in which the points of origin of the wave of electrical change are distributed over a definite portion of its length.

If the circumstances are such that the distribution of the points of origin cannot be determined, and if there is no means by which the electrical activity can be caused to originate at some definite point of

the bundle, then it does not appear that sufficient data are afforded by the electrometer records to discriminate between the effect of the non-coincidence of the points of origin, and of the gradual development and subsidence of the E.M.F. at any given point.

Inasmuch, however, as it must not be assumed that the rate of rise and rate of fall of E.M.F. are equal, *i.e.*, that the potential gradient at the wave-front is the same as that at the end of the wave, it becomes necessary to find some mode of representing the time relations of the variations of E.M.F. at any given point of a single linear conductor. For this purpose the following device may serve: Let each linear conductor be conceived as consisting of a number of parallel elements which flash into complete activity in succession, and remain active for a period not necessarily equal, after which each one in turn passes suddenly into a condition of rest; *e.g.*, let the conductor p_1 consist of parallel linear elements $\pi_0 \ldots \pi_n$ so that $\pi_1 - \pi_0$ is the small interval of time that elapses before the second element comes into action, and so on.

Then the time at which a difference of potential is derived from each of these elements in succession will be

$$t_{\pi_0}' = (p_1 - b + v\pi_0)/v, \qquad t_{\pi_1}' = (p_1 - b + v\pi_1)/v, \text{ etc.,}$$

and the times at which these contributions to the total P.D. cease will be respectively

$$t_{\pi_0}'' = [p_1 - b + v(\theta + k\pi_0)]/v, \qquad t_{\pi_1}'' = [p_1 - b + v(\theta + k\pi_1)]/v, \text{ etc.}$$

If $k = 1$ the formula represents an equal rate of rise and fall of E.M.F. at each point of the conductor.

If $k < 1$ the fall is more rapid than the rise.

If $k > 1$ the rise is more rapid than the fall.

C. *Effect upon the Variations of P.D. when the Conductors constituting the Bundle are not all of the same Length.*

There yet remains a farther complication arising in the case of a non-regular bundle, of which some of the constituent conductors do not extend far enough to pass under both leads.

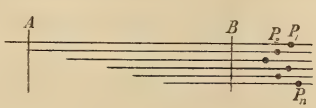


FIG. 11.—As in Fig. 9. A, B, electrometer leads. $P_1 \ldots P_n$, points of origin. But some of the linear conductors do not reach from B to A.

Let Q be the point at which a linear conductor ends, such that OQ = q, and following the notation hitherto employed, let $q_1, q_2 \ldots q_n$

represent the positions of the ends of the conductors in which a wave of electrical activity originates at the points p_1, p_2 . . . . p_n respectively.

Then, neglecting the function $f(\pi)$, which need not be here taken separately into account, the time of initial change of P.D. between the leads B and A will be $t = (p_1 - b)/v$ for the wave-front, which will continue moving onwards till $t_2 = (p_1 - q_1)/v$, where it will stop, so far as the conductor denoted by p_1 is concerned.

But the P.D. due to the wave-front in those conductors which do not reach the second lead, will not diminish until the tail of the wave, or subsidence of the E.M F., begins at the first lead. This will be when $t_1' = (p_1 - b + v\theta)/v$, and since this expression does not contain q, the second or electro-negative phase begins at the same time for regular as for non-regular bundles. But the second phase will end so far as the conductor denoted by p_1 is concerned when $t_2' = (p_1 - q_1 + v\theta)/v$, the relation $(p - q)/v$ determining whether there is zero between the phases or not.

It is evident, therefore, that in the case of a bundle of conductors some of which do not reach the second lead, the difference of potential due to the development of the E.M.F. will rise as rapidly as in a regular bundle, but will fall off more slowly, so that the first or electro-positive phase taken by itself may last longer and even be more intense.

But the second or electro-negative phase, due to the subsidence of the E.M.F. while beginning at the same time as in a normal bundle, will cease in each conductor as soon as it reaches q, causing the curve to tail off in proportion to the number of conductors which come to an end between A and B.

The function $f(\pi)$ merely rounds off the abruptness of the change.

III. *On the Interpretation of the Photographic Records.*

Gathering together the results of the preceding investigation, we find that the expression representing that portion of the P.D. due to the development of the wave of E.M.F. in the bundle is of the form

$$+\Sigma_{\mathrm{A}}^{\mathrm{B}} f(v, t, p, \pi, q),$$

and that due to its subsidence, is of the form

$$-\Sigma_{\mathrm{A}}^{\mathrm{B}} f(v, t, p, \theta, k \,.\, \pi, q),$$

the time relations of the total P.D. between A and B, being represented symbolically by the sum of these functions, taking t as the variable.

The object of the present investigation is to find out how far it may be possible under these exceedingly complicated conditions to determine

separately v, the velocity of propagation, p the distribution of the points of origin, θ the duration of the electromotive effect, π its rate of development, $k \cdot \pi$ its rate of subsidence, and q the influence of conductors which do not reach the second lead.

As has been already stated, the problem is rendered more difficult by the fact that there must always be an escape of current within the bundle from one conductor to another, the amount of which cannot be determined.

Velocity of Propagation.

The function v, *i.e.*, the velocity of propagation, can, as is well known, be easily determined by varying the interval between P and B. This may be done by shifting B with respect to A, or by changing the position of P.

And the value of v may then be deduced by comparison of the records obtained before and after the change.

The comparison may be made either between the times at which the curve commences, or between the times at which the direction of the movement of the meniscus changes.

The commencement of the curve is sometimes difficult to detect, otherwise it would be the best to use, for it depends simply on $f(b, p, \pi)$, and so long as p_1 is greater than b (*i.e.*, the points of origin are all outside the leads) the delay of the beginning of the first phase is directly proportional to the time required by the wave to travel from P to B.

But if B is brought within the region occupied by the points of origin, there is a discrepancy owing to the fact that the rate of rise of P.D. is altered, and lessened.

The apex of the curve is frequently very well defined. It is sometimes referred to as marking the end of the first phase and the beginning of the second. This is approximately but not strictly true. The change of sign occurs a short but variable time after the change of direction of the movement of the mercury.*

Taking however the time of the actual zero P.D., which can be easily determined by interpolation on the derived curve, it is necessary to inquire whether this varies directly with the distance of B from P.

Zero P.D. is reached when

$$+\Sigma_{\mathrm{A}}^{\mathrm{B}} f(v, t, p, \pi, q) - \Sigma_{\mathrm{A}}^{\mathrm{B}} f(v, t, p, \theta, k \cdot \pi, q) = 0.$$

Each term of this expression represents a series of values of P.D. forming a curve which is not symmetrical unless, among other conditions, the function $f(q)$ is absent.

* Hermann does not appear to have noticed that I had drawn attention to this fact in 1892. ('Phil. Trans.,' A, vol. 183, p. 103.)

But $f(q)$, whenever present, is conditioned by the position of A and B.

Consequently, if, to determine v, we compare the times of zero P.D. for different values of B, or of B and A, keeping P constant, the results will only be reliable if all the conductors of the bundle pass under A and B in both positions, but not otherwise, because then the shape of the second half of each curve will be altered, and consequently the time at which the ordinate of the second half of the curve of positive potential difference equals the ordinate of the first half of the curve of negative potential difference, will also be altered.

Hence, if the bundle of conductors is not regular in structure, v should be determined by keeping A and B constant, and varying P, the position of the exciting electrodes.

Having found v, a comparison of the lengths p, a, and b gives the true time t, and this compared with the measured time T shows whether there is any latent period S.

Influence of Temperature on Velocity of Propagation.

Is it possible by comparison of photographs taken at different temperatures, the position of the leads A and B and of the point of excitation P remaining constant, to determine the variation with the temperature of the velocity of propagation?

Here again there are two parts of the curve that might be selected for comparison, viz., the commencement and the apex. It is necessary to inquire whether $f(v)$ is the only function likely to be changed so as to affect the result.

The commencement of the curve depends on $\Sigma_A^B f(v, t, p, \pi, q)$.

Of these, $f(v)$, the velocity of propagation, and $f(\pi)$, the rate of development, are the only functions that can possibly be affected by change of temperature. But it is equally certain that they are both so affected, the E.M.F. developing much more slowly at low temperatures. (See fig. 12.) The determination therefore of the actual commencement of the curve by extrapolation on the analysis is liable to an error which in practice is negligible above 7° C., but increases rapidly below that temperature.

In our present apparatus, which will be fully described in another paper, temperature is determined to 0°·03 C., or 0°·01 C. if necessary. The importance of this precaution is evident from fig. 12.

If the apex is taken as the point of comparison, the conditions are more complex, for at the apex

$$+\Sigma_A^B f(v, t, p, \pi, q) - \Sigma_A^B f(v, t, p, \theta, k \,.\, \pi, q) = 0.$$

This will depend not only on $f(v)$ and $f(\pi)$, but also on $f(\theta)$ and $f(k)$, both of which may be affected by temperature.

Unless, therefore, it can be shown that these functions vary so as to preserve the same relative values at different temperatures, the apex cannot be used for this determination on a single set of observations.

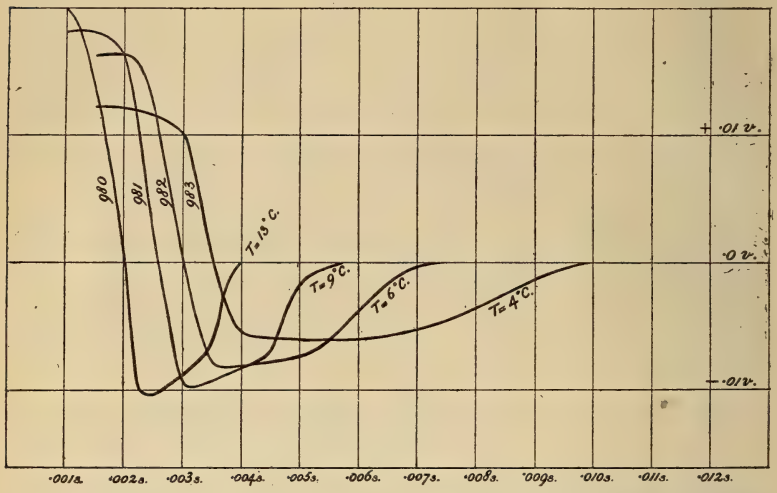


FIG. 12.—Influence of temperature on the response of a freshly prepared uninjured nerve. Electrometer leads 1 cm. apart, exciting electrodes 1·5 cm. from the nearest lead.

It should be noted that this method will not serve if the nerve is excited from the proximal end, because then $f(q)$ comes in.

If, however, the nerve is excited at two fixed points in succession at each temperature, keeping A and B constant, the effect of $f(\pi)$, $f(\theta)$, and $f(k)$ will be eliminated from the result, and only the function representing velocity of propagation will remain.

Functions affecting the Form of the Curve.

In the photographic records the total P.D. between the leads is given by

$$E = \alpha l + \beta . dr/d\theta,$$

where $l = r - R$ is the distance of the meniscus from its zero position R.

In the derived curves the values of E are plotted as ordinates with the corresponding values of time as abscissæ. Hence

$$dE/dt = \beta . d^2r/d\theta^2.$$

But by the preceding investigation it has been shown that, for the first phase,

$$\frac{dE}{dt} = f\left(\frac{dp}{dt}, \frac{d\pi}{dt}, \frac{dq}{dt}\right),$$

i.e., it is a function of three variables, namely, dp/dt, the number of linear conductors coming into action, $d\pi/dt$, the growth of the difference of potential at each point of each conductor, and dq/dt, the diminution of the number of active conductors between the leads owing to some of them not extending from P to A. Of these, dp/dt and $d\pi/dt$ can only be separated by such experimental methods as are tantamount to changing the distribution of the points of origin.

For instance, a muscle may be excited directly or by its nerve. In the latter case, the distribution of the points of origin is conditioned by that of the nerve endings—in the former case, by the disposition of the exciting electrodes. By a comparison of the results, a determination of $f(p)$ may be attempted, leaving, however, the uncertainty as to whether the altered mode of excitation may or may not have modified $f(\pi)$.

What may be termed the centre of gravity of the points of origin can easily be found on a gastrocnemius or sartorius by placing the leads one above and one below the nerve entrance, and shifting them till no excursion of the meniscus results on excitation.

Duration.

If the apex of the spike is very sharp and its beginning and end are gradual the analysis will show in many cases that the transition from maximum electro-positive to maximum electro-negative occupies about as long as the development of the first or electro-positive phase from zero. This is commonly met with in fresh muscle, especially gastrocnemius,* and fresh uninjured nerve.

It indicates that the duration of the active condition at each point is so related to rate of propagation, the development of the E.M.F. and its subsidence, the distribution of the centres of origin, and the distance between the leads, that the maximum slope of the wave-front (to make use of the other mode of expressing the problem) just reaches the electrode A as the maximum slope of the end of the wave passes B.

In other words, a sharp apex results from a particular relation between θ, v, p, k, π, and q, and the leads A and B, so that a sharp apex may become flat-topped from several quite different causes.

Flat-topped apices may be given by—

(1.) Kept nerves, especially at low temperatures.
(2.) Nerves which have been experimented on a great number of times.
(3.) Nerves under the action of CO_2. (See fig. 13.)
(4.) With leads further apart.
(5.) With leads closer together than usual.

* See my paper on the "Time Relations of the Capillary Electrometer," 'Phil. Trans.,' A, vol. 183, 1892, Plates 5 and 6.

It is very important to note that a flat-topped curve may indicate *either* that the wave of activity is so short compared with the distance between the leads, that its end has completely passed the first lead before its front has reached the second, *or else*, that the wave of activity is so long, that both leads are for an appreciable time included between its beginning and its end.

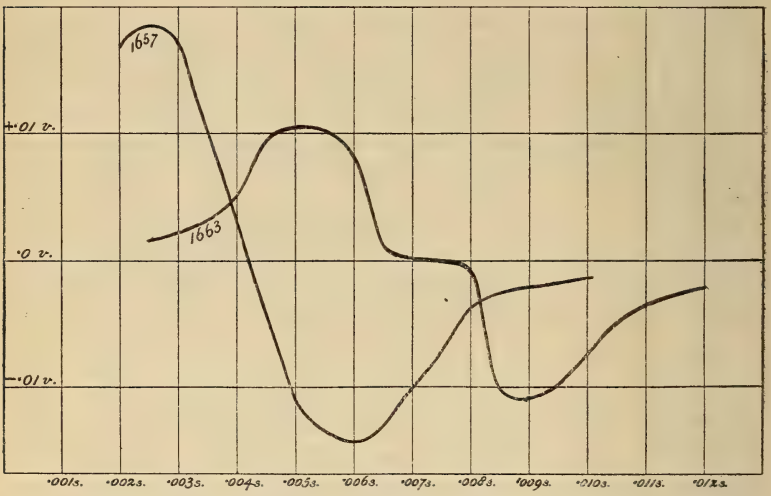


FIG. 13.—Effect of CO_2 on uninjured nerve kept 24 hours in tap-water saline No. 1657 = normal response in air, T = 4°·78 C. No. 1663 = response after CO_2, T = 4°·73 C. Electrometer leads 1·6 cm. apart, exciting electrodes 1·4 cm. from the nearest lead.

The most direct way of discriminating between the two cases is to alter the distance between the leads. If, with the leads farther apart, a sharper apex is produced, the wave is longer than from A to B. If, on the contrary, the length of the flattened summit is increased, the wave is shorter than the distance between the leads. An instance of this is given in fig. 14.

So far as the curves hitherto analysed have shown, both in kept nerves, and in nerve under the influence of CO_2, the length of the wave is relatively less than in fresh uninjured nerve. That is to say, the duration is not sufficiently increased to compensate for the slower rate of propagation.

In fig. 13 two analyses are given. One of these, No. 1657, is that of a nerve response under normal conditions. The apex of the photographed curve was sharp, indicating that the length of the wave was approximately equal to the distance between the electrodes, *i.e.*, 1·6 cm. The velocity of propagation, v, was 934 cm. per second.

The curve of which No. 1663 was the analysis, taken during the

action of CO_2, was flat-topped, indicating that the wave was shorter than the distance between the leads. Unfortunately, an exact determination of the velocity was not possible, owing to the great changes in the rate of development, $f(\pi)$. It may, however, be taken approximately as 467 cm. per second. This would imply a duration (θ) of 0·001713 second in both cases.

That is to say, the effect of CO_2 is to make the propagation rate slower, and the E.M.F. both smaller and slower in development, but not greatly to alter the duration of the maximum E.M.F. at any given point. With regard to the last clause, however, I propose to obtain more data.

Fig. 14 is an interesting set of analyses. The nerve, with the gastrocnemius attached, had been kept in tap-water saline for 20 hours. It was excited in the usual way, and the electrometer electrodes were placed for the first experiment, No. 1532, 21 mm. apart.

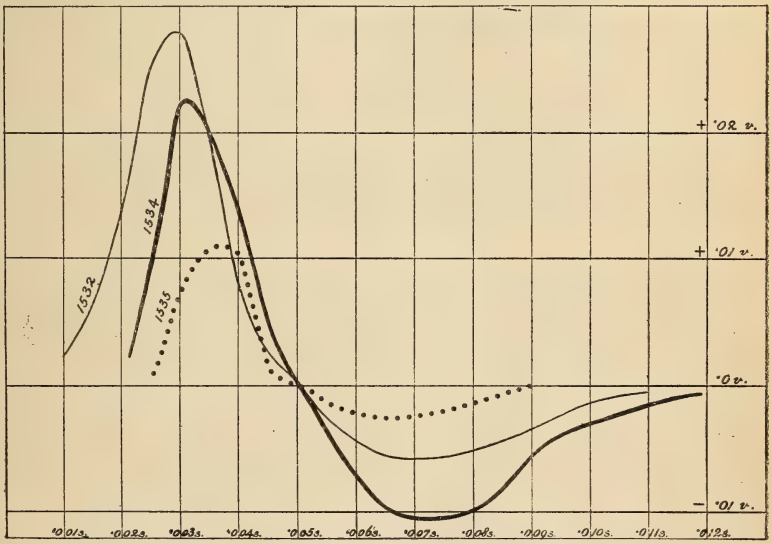


Fig. 14.—Uninjured nerve, kept 20 hours in saline. T = 4°C. No. 1532, distance between electrometer leads 2·1 cm. No. 1534, distance between leads 1·3 cm., approximately equal to length of excitatory wave. No. 1535, distance between leads 0·65 cm. Velocity 1055 cm. per sec. Duration about 0·00123 sec.

The proximal electrode was then moved 8 mm. nearer the distal, so that they were 13 mm. apart in No. 1534, and 6·5 mm. apart in No. 1535.

No. 1534 had a much sharper apex than either of the others, and the analyses indicate that the distance between the electrodes was greater than the length of the wave in 1532, and less in 1535. It may

be taken that the length of the wave was approximately 13 mm. The rate of propagation, v, was 1055 cm. per second, so that the duration (θ) was about 0·00123 cm. per second in this preparation.

In fig. 15 a similar preparation was used, but the nerve had been kept 48 hours.

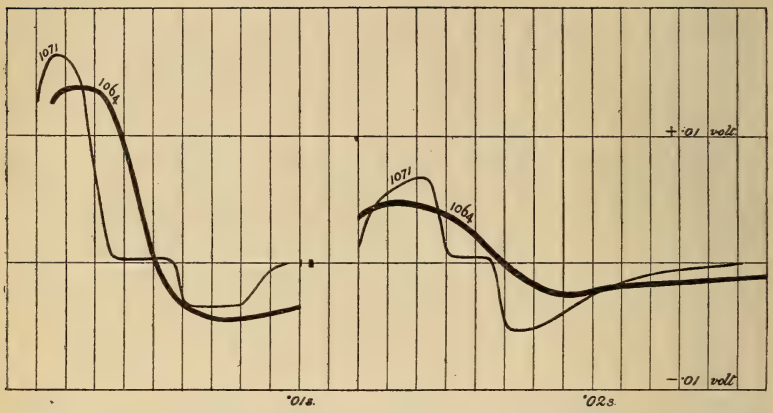


Fig. 15.—Influence of distance between leads on the time relations of the recorded electromotive changes. Nerve kept 48 hours. No. 1064, T = 4°·5 C., leads 2·3 cm. apart; second excitation at 0·0103 sec. No. 1071, T = 4° C., leads 0·8 cm. apart; second excitation at 0·01 sec.

On this occasion the proximal electrode was fixed, and the distal electrode was moved nearer to it, the distance between them being 23 mm. in No. 1064 (T = 4°·5 C.), and 8 mm. in No. 1071. It will be noticed that the curves in this case commence at the same time, whereas in fig. 14, when the proximal electrode was moved, they crossed the zero line together. The velocity, v, is about 1000 cm. per second, and the length of the wave about 2 cm., giving a duration θ of about 0·002 second, during which the electrical activity of each part in succession is at a maximum.

From the analyses it would appear that the period of development, $f(\pi)$, occupied 0·0005 second, and that of subsidence, $f(k.\pi)$ 0·0015 second, from which it may be inferred that this particular nerve would have been incapable of the smallest electrical response to a second stimulus during $f(\pi) + \theta$ = 0·0005 second + 0·0020 second, and only capable of a more or less feeble response during a farther period of 0·0015 second.

But although after about 0·0040 second the nerve might respond with full force, it by no means follows that a separate record would be given by the electrometer, for if the distance between maximum electro-negative of the first wave and maximum electro-positive of the

second exactly equals the distance between the leads, they neutralise each other, and the electrometer only records the electro-positive (first phase) of the first wave and the electro-negative (second phase) of the second, fusing them into a single response.

For the electrometer indicates in every case merely the algebraic sum of all the potential differences existing at any instant between the two leads.

Development and Subsidence.

Flat-topped curves are particularly valuable for determining the rates of development and subsidence of the electromotive condition at any given point on the nerve.

It is, however, necessary first to ascertain to which of the two classes mentioned above, the curve belongs.

(A.) Let the length of the wave be greater than the distance between the leads and let all the linear conductors pass under both leads.

Then both the beginning and the end of the first or electro-positive phase will be due to the wave front—passing first under the proximal and then under the distal electrode. The first phase of the photographed curve will therefore be sigmoid with the two ends similar as in fig. 16, *a*, and the analysis of it will be of the type shown in fig. 16, *b*, that is to say, symmetrical.

The second or electro-negative phase will be due entirely to the subsidence of the electromotive condition, first at the proximal lead, giving the beginning of the second phase, and then at the distal lead, corresponding to the end of the curve.

This phase also will be sigmoid with the two ends similar. The shape of each end will depend on two functions, one of which, $f(p)$, representing the distribution of the points of origin, is common to both first and second phase; but the other, $f(k, \pi)$, the rate of subsidence, is peculiar to the second phase.

I have come to the conclusion that in the great majority of cases the curves indicate a slower rate of subsidence than of development. The second phase of the photographed curve may therefore have under these conditions the form shown in fig. 16, *c*, and its analysis will be represented by fig. 16, *d*.

(B.) The second class of curve is produced when the length of the wave is less than the distance between the leads. In this case the beginning of the first phase and the beginning of

the second phase are both due to the development of the electromotive changes, and the end of the first phase and the end of the second phase to their subsidence. Thus a quick development and slow subsidence will be indicated for this class of curve when the end of each phase is more gradual than its beginning.

It is interesting to compare these results with those obtained by Professor Gotch and myself from Malapterurus.*

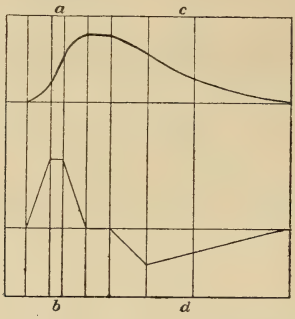


FIG. 16.—Diagram of record in which rate of subsidence is slower than rate of development. a = first phase of record; b, its analysis. c = second phase of record; d, its analysis.

Fig. 17 represents the analyses of three single shocks, the first of which was given when the preparation was fresh, and the third just before it ceased to respond to a stimulus. It will be observed that the development of the E.M.F. occupies from one-third to one-half the time required for its subsidence, and that the duration of the maximum is relatively short.

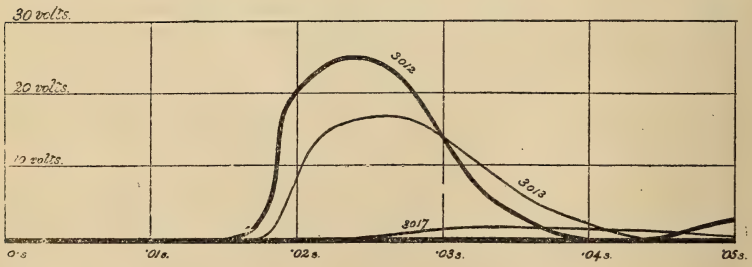


FIG. 17.—Analysis of three single shocks of piece of electrical organ of Malapterurus, taken at intervals of several minutes. T = 5° C. No. 3012, leads 1·5 cm. apart; No. 3013 ditto, but preparation failing. No. 3017, leads 0·2 cm. apart. Preparation ceased to respond after this.

* 'Roy. Soc. Proc.,' vol. 65, p. 439.

And analyses of other photographs—of which examples were given in our paper—show that no new development of E.M.F. takes place until the previous one is over.

The electrical phenomena of excited nerve and those of the electrical organ of Malapterurus are thus similar as regards these two points, namely, (1) the relative duration of development and of subsidence; (2) incapacity to show a second electromotive change while the first is in progress.

Farther confirmation is thus afforded of Professor Gotch's view, that the electrical phenomena of such fish are of nervous origin.*

Elimination of Fibres which do not reach the Second Lead.

The function $f(q)$ may be investigated by taking advantage of the fact that a nerve may be excited at either end. If two pairs of exciting electrodes are placed, one at each end of a long nerve, and a pair of electrometer leads between, as far apart as the length of the preparation will admit, the resulting curves will differ according to which end is excited. For when the electrodes at the proximal end are used, every fibre of the nerve is excited, but in the other case only those that reach the distal end are affected. Accordingly, the latter curve though smaller, is more symmetrical, and represents a simpler condition, from which $f(q)$ is eliminated.

When the proximal end is excited, as in the majority of experiments, the results are curiously complicated by conditions depending on the rate of propagation, the distance between the electrodes, and in addition their actual position on the nerve. For the ends of the fibres are not evenly distributed along the nerve, but occur in groups wherever a branch has been cut, and the effect of some of these groups can occasionally be distinctly recognised in a series of curves from the same preparation.

When the distal end is excited the resulting curves confirm the conclusion arrived at from my analyses that the development of the E.M.F. at any point is more rapid than its subsidence. A typical illustration of this is given in fig. 18, the analysis of the response of a kept nerve, with leads 3·1 cm. apart, at a temperature of 5° C.

A complete interpretation of this analysis cannot be given, as the commencement of the response is masked by the movement of the meniscus due to escape of the exciting induction shock, and there are not sufficient data to determine the rate of propagation, v, with accuracy.

But from other examples it may be inferred that the length of the wave was less than the distance between the electrodes. This is con-

* See Schäfer, 'Text Book of Physiology,' vol. 2, Article by Gotch, "On the Physiology of Electrical Organs," p. 591. Edin. and London, 1900.

firmed by the shape of the curve, which shows a zero pause of about 0·0005 second, during which time the wave must have been wholly between the electrodes.

Accordingly the rise of the electro-positive potential difference is due to the passage of the wave-front (development of E.M.F.) past the first electrometer lead, and the fall of it to the end of the wave (subsidence of E.M.F.) passing the first lead.

Similarly the first part of the second or electro-negative phase is due to the arrival of the front of the wave at the second lead, and the second part to the cessation of the E.M.F. under the second lead. Hence it may be inferred since (q) is eliminated, that the distance from maximum electro-positive to maximum electro-negative represents the time

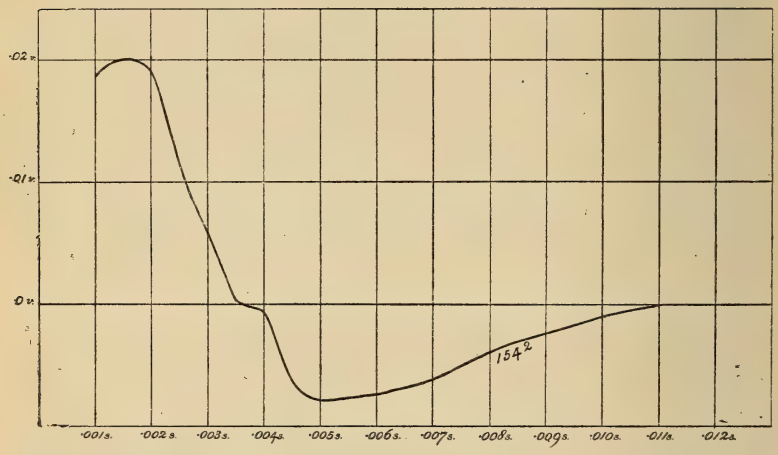


FIG. 18.—Nerve excited at distal end to eliminate effect of nerve fibres which pass only under one lead. T = 5° C. Leads 3·1 cm. apart.

required for the response to travel from one lead to another. This gives a velocity, v, of about 1000 cm. per second, and since there is a zero pause of at least 0·0005 second, the whole wave of electrical response from beginning to end cannot have been more than 2·6 cm. long, and the duration of the electrical effect at each point may have been about 1/400 second.

It should be noticed that the first part of the electro-positive phase and the first part of the electro-negative last a much shorter time than the end of the electro-negative and the end of the electro-positive. This agrees with what has been already stated about the rapid development and slow subsidence of the E.M.F. The shape of the second phase indicates that the nerve had been slightly injured near the proximal lead. The smaller E.M.F. is partly accounted for by the fact that although f (q) is eliminated, the *short-circuiting due to the*

unexcited nerve-fibres which surround the active fibres near the proximal end, shunts part of the current.

This curve should be compared with No. 1535 in fig. 14, in which the wave of electrical response is *longer* than the distance between the leads, and consequently the first phase is due entirely to the development, and the second to the subsidence, of the E.M.F.

The material discussed in this paper consists mainly of some 1900 photographs of the electrical response of nerve, taken in the Physiological Laboratory, Oxford, by Professor Gotch and myself. I have made full analyses of more than 150 of the curves, and have measured the principal points of a much larger number.

Many other examples could be given, but I have in each case selected the one best suited, either from the sharpness of the definition or the completeness of the data, to illustrate the theory. It has become evident from a comparison of the photographs, that the values of v, θ, π, and $k\pi$, are greatly affected by temperature and the condition of the preparation; but as these involve the physiological side of the problem, which will be dealt with by Professor Gotch, I have for the present confined myself to showing the methods by which they may be determined.

"Contributions to a Theory of the Capillary Electrometer. I.—On the Insulation Resistance of the Capillary Electrometer, and the Minimum Quantity of Electricity required to produce a Visible Excursion." By GEORGE J. BURCH, M.A. Oxon., F.R.S., Lecturer in Physics, Reading College, Reading. Received April 17,—Read May 1, 1902.

What may be called the Insulation Resistance of the capillary electrometer is important for two reasons—first, as to its bearing on the theory of the instrument, and secondly, as affecting the method of using it in dealing with electrical charges or quantities of limited amount. I propose briefly to record some of my own experiments on this head.

In many capillary electrometers, if an excursion of the meniscus is produced by touching the terminals with a source of electromotive force and then removing it, leaving the circuit open, the meniscus returns in a comparatively short time to the position it would occupy if the instrument were short circuited. In other words, the charge, which, as Lippmann showed, is contained in the instrument as long as the meniscus is deflected from its zero position, gradually leaks away. The question naturally arises, whether this leakage is accidental like

that of a gold-leaf electroscope, or essential, in the sense that some small current may be necessary to maintain a deflection. Lippmann, in his original thesis, did not mention any direct experimental investigation of this problem, though his whole mathematical argument is in accordance with the supposition, that no current is required to maintain a deflection. I therefore began my practical examination of the properties of the capillary electrometer in January, 1887, with this experiment. I made a quick-acting electrometer, carefully insulating its parts with freshly cut soft rubber, and joined it up in series with a very sensitive galvanometer in the derived circuit of a potentiometer. Both instruments were furnished with short-circuiting keys. On opening the electrometer key, there was a large excursion of the meniscus. As soon as it had come to rest in its new position under the action of the electromotive force, the galvanometer short-circuiting key was opened. There was no deflection. But any thing that caused the meniscus to *move*—whether a slight change in the pressure on the mercury, or a small movement of the rider of the potentiometer, produced a large or even violent, but always temporary, deflection of the galvanometer.*

I found afterwards that Fleischl had obtained similar results in 1879.

The converse of this experiment is specifically referred to by Lippmann. If a charge is communicated to an electrometer by rubbing the terminals with the finger, and the circuit is left open, the mercury returns to its zero point very much more slowly than if the circuit is closed. And I found that by careful attention to the insulation the capillary electrometer could be made to hold a charge quite as long as a gold-leaf electroscope. Thus another instrument made a few weeks later was left charged with the circuit open, and "no change in the deflection could be detected after 35 minutes."

The most remarkable experiment of this kind was made unintentionally. In June, 1898, I made a new projection electrometer for the work on the electromotive properties of nerve in which Professor Gotch and I are engaged at Oxford. This instrument is the most rapid and sensitive that I have ever employed, and it is still in use. It is a modification of that figured in my book.† The capillary, which points vertically downwards, is contained in a glass trough about $\frac{1}{2}$ mm. wide and 25 mm. long, formed by grinding away the front of a thick-walled tube so as to lay open the bore from end to end. This trough is closed in front by a piece of glass, and its lower end dips into

* A galvanometer by Elliott, of 25,500 ohms resistance. The current given by one Daniell cell through a resistance of 63,900 megohms produces a deflection of one scale-division, according to the maker's certificate.

† "The Capillary Electrometer in Theory and Practice," reprinted from the 'Electrician,' 1896.

dilute sulphuric acid, the surface tension of which causes it to fill the trough to its upper end. The acid is contained in a U-tube, the bend of which is filled with mercury. Two platinum wires, dipping respectively into the mercury in the U-tube and that in the capillary, serve to connect the electrometer with the experimental circuit. A few weeks after it was set up, it was accidentally left, charged to about 0·025 volt, with the circuit open from 1 P.M. on Saturday to 10.30 A.M. on Monday. The image of the meniscus was still, as it had been left 45½ hours before, between two reference lines on the screen corresponding to a difference of potential of 0·0008 volt, so that this represents the maximum loss, and as the zero-position had not altered, at least 97 per cent. of the original charge still remained in the electrometer. And as the circuit included no less than seven keys and switches and about 23 metres of wire, it is probable that the instrument was not responsible for all the leakage. Inasmuch as it might be objected that the mercury had stuck in the tube and so maintained its position before closing the circuit, I waved an electrified ebonite tube to and fro near one of the terminals. This causes the meniscus to move up and down by induction exactly as it would cause the gold leaves of a charged electroscope to diverge more widely or collapse. But just as the gold leaves would remain divergent after the final removal of the ebonite at the same angle as when they were first charged, so the meniscus when the ebonite is taken away returns to the position it had assumed in virtue of the original charge put into it, always supposing that no sparks have been allowed to pass. This method is extremely useful in guarding against false readings due to a sticky tube.

I have verified this result several times since by observations extending over 5 or 6 hours, but I have not cared to risk leaving the key open during my absence from the room.

After a lapse of nearly four years the insulation resistance is naturally less, but it varies greatly with the weather. The easiest way of making comparative measurements is to observe the time required for any charge to fall to half its initial difference of potential. This I shall refer to briefly as the "time of half-discharge," and it answers to the "time constant" of a condenser, but is simpler to use in practice, as it is easier to divide a number by 2 than by 2·71828, and also to observe deflections consisting of a whole number of scale divisions.

On March 27th of this year, the time of half-discharge was 13 minutes. On April 14th, just before rain, the weather having suddenly become warmer, the time of half-discharge was only 65 seconds. This is an exceptionally low value. A gold-leaf electroscope in the same room could not be made to retain a charge for 10 seconds.

In dry weather the electrometer responds so readily to frictional charges that the greatest care has to be taken to avoid touching the table with the sleeve or coat, lest the mercury should be driven out at the tip of the capillary.

The cause of the leakage is obviously two-fold. Part of it is external, as is evident from the marked influence of the weather. The capillary electrometer is necessarily a difficult instrument to insulate. Glass is under the most favourable circumstances liable to attract moisture in a rising temperature, and it becomes still more so when a portion of its surface is in contact with sulphuric acid diluted to such an extent. Varnish cannot be used, but I have thought vaseline round the ends of the tubes diminishes the leakage.

The other part of the leakage is internal. The cause of it is easily explained. The acid wets the glass and the mercury does not. There is therefore a tendency for the acid to insinuate itself between the mercury and the walls of the capillary. That it does so is evident on examining the mercury column under the microscope by front light with a high power. A film of liquid can be seen between the metal and the glass, and traced for some distance. When a short column of mercury 2 or 3 cm. long is used, this film soon passes right up the tube, and in a few weeks acid can be seen above the mercury. Even in the U-tube, where the mercury is beneath the acid, the same action takes place, though less rapidly. For this reason I prefer to have a depth of 8 or 10 cm. of mercury in the capillary, and 5 cm. in the U-tube. The internal leakage is least when the electrometer is new. Instruments which leak badly generally also "creep," *i.e.*, if left on open circuit the mercury does not remain at zero, but creeps slowly up or down, owing to some electomotive force within the instrument.

It seems at first sight inconceivable that there should be no current through a circuit consisting of platinum—mercury—dilute sulphuric acid—mercury—platinum, all of which are good conductors. Yet we have this fact: the time of half-charge of a quick electrometer is of the order of $\frac{1}{50}$ second, but the time of half-discharge of the same instrument on open circuit may be counted by days. The resistance to the passage of the current is manifestly not ohmic, but some effect produced at the interfaces between mercury and dilute acid.

The conditions suggest the counter electromotive force of polarisation, and here we are met by the experiments of Bouty, showing that the sum of the electromotive forces of polarisation at the two electrodes is always less than the applied electromotive force, no matter how weak it may be, so that there is always a permanent current through the electrolyte.

The explanation is I believe to be found in the fact that we are dealing with an interface between two liquids which cannot diffuse into each other, and that the electrical and chemical as well as

mechanical stresses are in some way distributed evenly over the whole area of contact; so that local action, to which perhaps we should ascribe the permanent current through the electrolyte with solid electrodes observed by Bouty, is prevented. I have already published my opinion that there is no electrolysis, properly so called, in a well-made electrometer.

A point of practical interest arises in connection with the rapid leakage of the charge in damp weather. How far does this defective insulation affect the records obtained with the apparatus? Taking the worst conditions, namely, when the time of half-discharge was 65 seconds, and the severest test, namely, photographing the discharge of a small Leyden jar charged to half a volt into the electrometer, we have the following data: The passage of the sensitive plate occupies 0·1 second, *i.e.*, $\frac{1}{650}$ of the time of half-discharge. A simple calculation will show that the total loss of charge in 0·1 second would be about one-tenth per cent., and as such a record would be completed in less than 0·01 second, the difference would be quite inappreciable.

It may be of interest to give some data as to the quantity of electricity which an electrometer will detect.

I took an ordinary gold-leaf electroscope with a brass knob 1 inch in diameter, and charged it so that the leaves diverged at an angle of about 20°. I then touched one of the terminals of the electrometer with the knob of the electroscope—the mercury instantly shot right out of the field. I then tried smaller charges, and found that a permanent excursion of fully 1 cm. was caused by a charge that produced a barely visible divergence of the gold leaves.

In order to measure the minimum quantity of electricity required to cause a visible movement of the meniscus, I earthed one pole of a four-cell accumulator, and touched the other with an insulated brass ball 3·3 cm. diameter, freshly polished, but not lacquered. On touching one of the terminals of the electrometer with the ball thus charged, there was a sudden upward jerk of the meniscus, not followed by any return. After six such charges, the level of the image of the meniscus had risen fully 1 mm. On reversing the charges on the sphere, the direction of the movement of the meniscus was reversed. With 4 volts the excursions were smaller, but well marked, and with 2 volts they were just visible. The total quantity of the charge therefore was $\frac{3\cdot3}{2} \times \frac{2}{300} = 0\cdot011$ electrostatic unit. But as the screen on which the excursions were observed was placed 81 cm. from the lens, whereas the photographs are taken at a distance of 126 cm., and as details are discernible on the photographs with a lens that are quite invisible to the naked eye, it may be safely said that a quantity equal to $\frac{1}{100}$ electrostatic unit will produce a measurable excursion.

The capacity of this electrometer, measured by the method of mix-

tures of Lord Kelvin, using a standard microfarad condenser, and employing the electrometer itself as indicator, is 0·363 microfarad at the part used in this experiment.

"On a Peculiarity of the Cerebral Commissures in certain Marsupialia, not hitherto recognised as a Distinctive Feature of the Diprotodontia." By G. ELLIOT SMITH, M.D., Ch.M., Professor of Anatomy, Egyptian Government School of Medicine, Cairo, and Fellow of St. John's College, Cambridge. Communicated by Professor G. B. HOWES, F.R.S. Received March 5,—Read March 20, 1902.

It has been known for a considerable time that some of the fibres of the ventral commissure of the cerebrum in certain Marsupials dissociate themselves from the rest of the commissure as soon as they have crossed the mesial plane; and that, instead of passing bodily into the *external* capsule, which is the usual course of the fibres of the ventral or anterior commissure, they form an aberrant bundle which associates itself with the *internal* capsule so as to reach the dorsal area of the neopallium by a shorter and slightly less circuitous course (fig. 2).

This peculiarity was represented in the drawings of sections through the brains of *Macropus* and *Phascolomys*, in 1865, by the late W. H. Flower.* It was more distinctly shown in a diagram† illustrating a coronal section through the brain of a Derbian Wallaby which was published 27 years later by Johnson Symington. Two years later I placed on record the observation upon it, that "in *Phalangista* [*Trichosurus vulpecula*] a bundle of anterior commissure fibres proceeds to the cortex *viâ* the internal capsule, in addition to the external capsule,"‡ and in the same place noted an analogous arrangement in various species of *Macropus*.

In 1897 Theodor Ziehen recorded§ the presence of such fibres in *Macropus*, *Aepyprymnus*, and *Phascolarctus*; but, like Flower and Symington before him, he did not venture on any explanation of them.

* "On the Commissures of the Cerebral Hemispheres of the Marsupialia and Monotremata, as compared with those of the Placental Mammals," 'Phil. Trans.,' vol. 155 (1865), p. 633.

† "The Cerebral Commissures in the Marsupialia and Monotremata," 'Journal of Anatomy and Physiology,' vol. 27, 1892, fig. 3, p. 81.

‡ "Preliminary Observations on the Cerebral Commissures," 'Proc. Linn. Soc. of N.S.W.,' 1894, pp. 647—648.

§ "Das Centralnervensystem d. Monotremen und Marsupialia (Semon's Zoologische Forschungs-Reisen in Australien)," 'Denkschr. Medic.-naturwis. Gesellsch. Jena,' vol. 6, Lf. II and IV, 1897—1901.

The investigations for my memoir of 1894 were carried out chiefly on the brains of *Ornithorhynchus*, *Perameles*, *Trichosurus*, and *Macropus*.

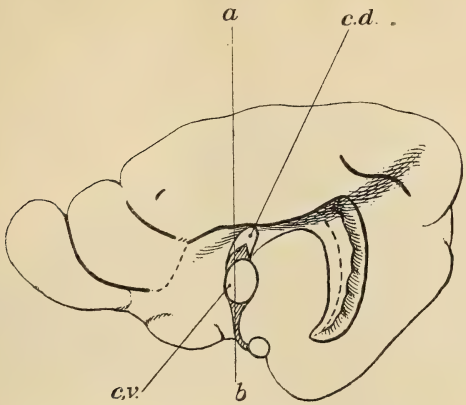


FIG. 1.—*Trichosurus vulpecula.* The mesial aspect of the right cerebral hemisphere. × 2.

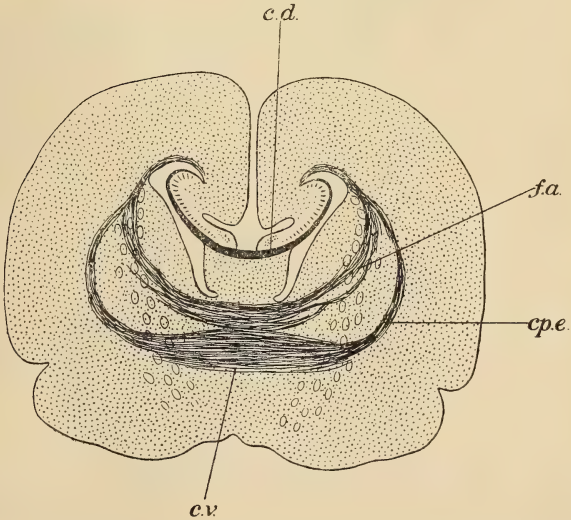


FIG. 2.—Transverse section through the two cerebral hemispheres of the same in the plane *a*, *b* (fig. 1). × 3.
c.d., commissura dorsalis. *c.v.*, commissura ventralis. *cp.e.*, capsula externa. *f.a.*, fasciculus aberrans.

In the Monotreme and the smaller Marsupial (*Perameles*) the common Mammalian relationship of the ventral commissure to the external

capsule was found to obtain, but in the two larger Marsupials some fibres of the ventral commissure were found to pursue the aberrant course indicated above. It was perhaps not unnatural to suppose (as I did in that early attempt at interpreting this peculiarity) that the increased size of the neopallium in *Trichosurus* and *Macropus* was wholly responsible for the presence of this aberrant bundle. For it seemed that since the commissural fibres of the neopallium had become too abundant to be wholly accommodated by the path provided by the external capsule, they, so to speak, had overflowed into the internal capsular route.

Upon examining a much larger series of Marsupials than were available when my memoir of 1894 was written, I soon became convinced that the explanation of the causation of this peculiarity which I then suggested could not be regarded as alone sufficient. I found the aberrant bundle in all members of the genera *Macropus*, *Halmaturus*, *Hypsiprymnus*, *Dendrolagus*, *Trichosurus*, *Petaurus*, *Phascolarctus*, and *Phascolomys*, quite irrespective of the size of the brain and of the extent of the neopallium. On the other hand, I sought in vain for it in *Perameles*, *Sarcophilus*, *Dasyurus*, *Sminthopsis*, *Didelphys*, *Myrmecobius*, and *Notoryctes*, even though many of these genera possess larger brains than some of the Diprotodonts.

These facts seemed to suggest that the aberrant bundle was essentially a distinctive feature of the Diprotodont Marsupials, and it appeared to me that the crucial test of this hypothesis would be afforded by the examination of the brain of *Thylacinus*, which, although that of a Polyprotodont, is almost, if not quite, as large as the brain of the largest Macropod, and considerably larger than those of all other living Diprotodonts. I accordingly submitted the cerebrum of *Thylacinus* to the test, and found no trace of the aberrant bundle (figs. 3 and 4), *wherefore it is clear that the presence of this aberrant fasciculus of the ventral commissure is distinctive of the Diprotodontia.*

If we compare the brain of the Diprotodontia with that of the other three Mammalian groups: Monotremata, Polyprotodontia, and Eutheria, the meaning of the aberrant bundle becomes, I believe, fairly obvious.

A study of the structure of the brain in the Monotremes and the Polyprotodont Marsupials shows that in the progenitor of the Mammalia all the commissural fibres of the neopallium must have passed into the ventral commissure *viâ* the external capsule (fig. 4).

The most pronounced growth tendency in the earliest Mammals must have been the enormous increase of the extent of the neopallium, for while at the beginning of the Eocene period this was almost as insignificant as it is in the Reptilia, in most recent Mammals it attains a bulk which far exceeds that of the whole of the rest of the nervous system. This sudden expanse of the neopallium would lead to the

development of an enormous mass of fibres which must find some outlet from the pallium. There are only three possible routes for

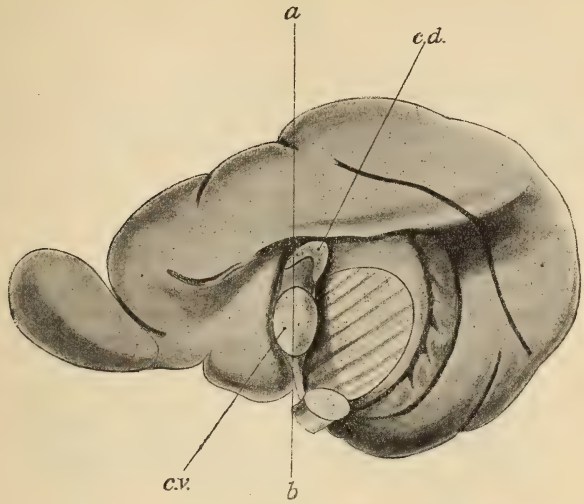


FIG. 3.—*Thylacinus cynocephalus.* The mesial aspect of the right cerebral hemisphere. Nat. size.

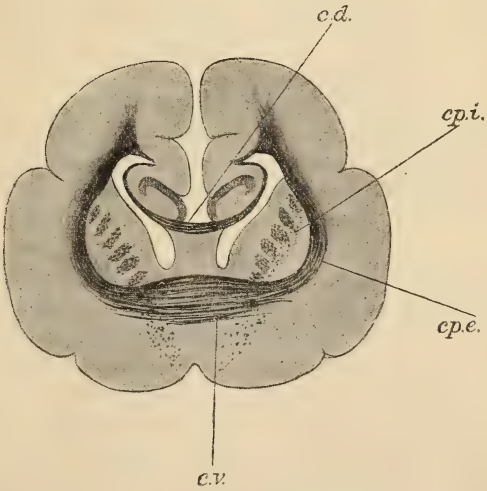


FIG. 4.—Transverse section of the two cerebral hemispheres of the same in the plane *a*, *b*. (fig. 3).

cp.i., capsula interna. Other references as for fig. 2.

commissural fibres of the neopallium to the mesial plane. There is first of all the external capsule, which chiefly consists in all Mammals of such fibres passing to the ventral commissure: we find the second route in the path mapped out by the internal capsule from the dorso-lateral neopallial area to it; and the third route can only involve the invasion of the alveus of the hippocampus.

These three routes, by which a fibre coming from the dorsal neopallium in the region x (fig. 5) may attain the region y in the other hemisphere, are indicated schematically in the following diagram:—

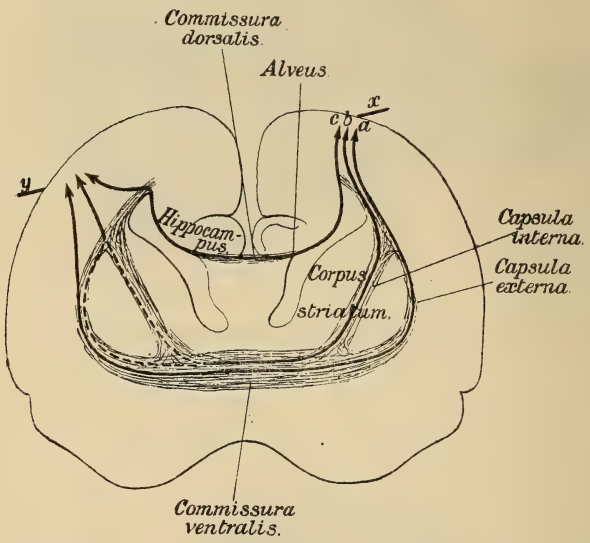


Fig. 5.—A scheme of a transverse section in the same plane as that represented in figs. 2 and 4 to show the three routes a, b, and c by which a commissural fibre may pass from the point x in one hemisphere to the region y in the other in different mammalian brains.

All the neopallial commissural fibres in the Polyprotodontia and some only of these in the Diprotodontia and Eutheria follow the first, which is also the primitive, route (a). The commissural fibres, which spring from the dorso-lateral region of the neopallium in the Diprotodontia seem to be crowded out, as it were, of the first route and pursue the second route (b). In the Eutheria the neopallial commissural fibres from the dorso-lateral region of the hemisphere forsake both the first and second routes and break through the hippocampal formation (c), or, in other words, invade the alveus so as to form a new dorsally situated neopallial commissure which is the corpus callosum.

This hypothesis of the origin of the corpus callosum I have pre-

viously stated in my memoir of 1894 (*vide supra*) and I discussed it more fully in 1897.*

I refer to the matter now, merely to point out that the same determining cause which in the Eutheria calls the "corpus callosum" into being is probably functional in bringing into existence the "aberrant bundle" in the Diprotodontia.

When the relations of these commissural bundles in the four divergent mammalian groups—Monotremata, Polyprotodontia, Diprotodontia, and Eutheria—are carefully studied we are able to appreciate one —and by no means the least—of the reasons why the Eutheria have attained such a pronounced ascendency over the other three groups.

Their brain is that which has retained that particular modification of the commissural arrangement which not only furnishes the shortest and most direct path of communication (*c*) between the two hemispheres, but also permits of an unimpeded expansion (which is so freely exercised by the corpus callosum). In the other three groups, in which all the neopallial commissural fibres pass through the ventral commissure, the undue expansion of the latter would produce considerable disturbance in the surrounding structures, which in turn would exercise a restraining influence upon any marked increase in size in the commissure itself.

The development of any such commissural mass as the corpus callosum of the more highly organised Mammalia in the position occupied by its homologous fibres (fig. 3, *a* and *b*) in the Monotremes and Marsupials would cause the most profound disruptions of the corpus striatum, optic thalamus, and the basal region of the brain, and the complete disorganisation of its whole.

For these various reasons the development of the corpus callosum gives the Eutherian brain a great advantage in the struggle for supremacy, which must have exercised a considerable if not predominant influence in making the Eutheria the highest Mammals.

"A New Interpretation of the Gastric Organs of Spirula, Nautilus, and the Gastropods. By J. E. S. Moore and W. B. Randles, B.Sc. Communicated by Professor G. B. Howes, F.R.S. Received March 17,—Read April 24, 1902.

(From the Zoological Laboratory, Royal College of Science, London.)

Related to the stomachs of some Gastropods and Lamellibranchs there are two conspicuous appendages, the so-called crystalline style-sac and the so-called spiral cæcum.

* "The Origin of the Corpus Callosum," 'Trans. Linn. Soc. of London,' 2nd series, Zoology, vol. 7, part 3, June, 1897, p. 61.

They have long been known to exist in these orders, but have never been adequately dealt with from a morphological point of view. Both have hitherto been regarded as enigmatical structures, capricious in their appearance or non-appearance, and in their relationships.

The first of these organs, the so-called crystalline style-sac, which characterises the stomach of some Lamellibranchiata, was shown by Collier* in 1829 to be present also in several Gastropods. These results were subsequently confirmed by Huxley† in the case of Pteroceras, and they have much more recently been both confirmed and extended by Martin Woodward‡ and one of us.§

From these observations, there seems to be little room left for doubt, that when present, the style-sacs both of the Lamellibranchs and the Gastropods are homologous, and their appearance in two such widely separated groups indicates that they are of considerable antiquity. Or in other words, the species which possess these organs are primitive in that respect. The same may be said of the spiral cæcum, for it occurs as an appendage of the stomach in many forms of rhipidoglossate Gastropods, and it is also found in relation to the gastric tubes of the Cephalopods. Until lately the spiral cæcum had not been observed in any but the most primitive of rhipidoglossate Gastropods, such as Pleurotomaria and Haliotis, but it is present in those forms of Rhipidoglossa where one gill has become suppressed, such as Trochus, and quite recently it has been shown by one of us|| that both the spiral cæcum and the style-sac occur together as appendages to the stomach of the prosobranch Nassopsis. Still more recently this same association of style-sac and cæcum in the gastric apparatus of the Prosobranchiata has been found to exist in the genera Limnotrochus and Chytra,¶ both of

* "General Observations upon Univalves," printed in the 'Edinburgh New Philosophical Journal' for 1829, p. 231.

† Speaking with wonderful acumen, when we think of the date—1853—Huxley says:—"Of the pyloric sac—this appears in various forms in a great number of the Mollusca, and seems to be always in a special relation with the liver. In Atlanta it has been seen that its glandular parietes form the liver. In the Cephalopoda, the hepatic ducts enter its representative, the spiral sac of Octopoda, the elongated sac of Loligo.

"In Pteroceras a very remarkable structure exists, which, so far as I am aware, has not yet been noticed. The existence of a crystalline style in connection with the alimentary canal, has long been known in the Lamellibranchiata, but it has hitherto been supposed to be confined to them. However, in Pteroceras, the pyloric sac contains a very complete style." T. H. Huxley "On the Morphology of the Cephalous Mollusca," 'Phil. Trans.,' vol. 143, 1853, p. 60.

‡ 'Proc. Mal. Soc.,' vol. 1, p. 143, 1893—1895.

§ J. E. S. Moore, "The Molluscs of the Great African Lakes," 'Quart. Journ. Microsc. Sci.,' vol. 41, p. 199, 200.

|| J. E. S. Moore, "The Molluscs of the Great African Lakes, Nassopsis and Bythoceras," 'Quart. Journ. Microsc. Sci.,' vol. 42, p. 190.

¶ Miss Digby, 'Journ. Linn. Soc., Zool.,' vol. 28, p. 434.

which, curiously enough, like Nassopsis, are members of the old marine fauna of Lake Tanganyika, and it also occurs in the genus Turritella.*

It has often been stated that the crystalline style, and in consequence presumably its sac, occurs generally among the Rhipidoglossa, but so far as we have been able to ascertain, this is not the case; at any rate, the style-sac does not occur in Pleurotomaria, Trochus, Turbo, or Haliotis.

Nassopsis thus presents us with a stomach in which these appendages are more completely developed than in the Rhipidoglossa, and in this genus the gastric apparatus has the following relationships:—

The œsophagus leads into the stomach in the manner represented in fig. 1, *œ*. In the gastric chamber into which it opens there are several conspicuous glandular folds, and two of these (*s.f.*) which lead away from the œsophageal aperture contain between them the opening of a conspicuous "bile duct" (*b.d.*). As they recede from the œsophageal aperture they approximate together and become related to a complex spiral organ (*sp. c.*) which projects beyond the parietes of the stomach and constitutes the so-called spiral cæcum. At the opposite end of the stomach and constricted off from it by a raised annulus, there is another thick-walled diverticulum which is lined with a shining cuticular membrane; this is the style-sac, and in Nassopsis it generally contains a semi-transparent mass of secretion, the so-called crystalline style (fig. 1, *c.s.*). Thus of such a stomach it may be said that the intestinal and œsophageal apertures are related to a proper stomachic chamber which possesses two chief diverticula, on the one hand the style-sac, and on the other a complex spiral organ in definite relation to the principal bile duct, the spiral cæcum.

It has generally been assumed that the gastric apparatus of a Cephalopod differs from, and is more complex than, that of the Gastropods. But keeping the above condition of the gastric apparatus in Nassopsis in mind, we find that in Nautilus (fig. 2, or in Sepia, fig. 3) the œsophagus after becoming dilated into an expansion known as the "crop" (fig. 2, *c.r.*), leads into a narrow folded tube (*s.t.*). This tubular portion is connected with a diverticulum, the so-called "gizzard" (*c.s.*) of the Cephalopods, but which like the crystalline style-sac of the Gastropods has thick walls, and is lined inside by a folded cuticular membrane (*c.u.*). In Nautilus, moreover, the so-called gizzard is constricted off from the stomach proper by a thickened annulus, corresponding to that which separates the style-sac of the Gastropods from the stomach proper. The gizzard of Nautilus is in fact a diverticulum of the stomach, and has exactly the same general structure as the style-sac in Nassopsis.

* W. B. Randles, 'Anat. Anz.,' 1902. Bd. xxi, p. 201.

With the exception of *Turritella communis*, both style-sac and cæcum have hitherto only been found in association in the stomachs of the Halolimnic Gastropods of Tanganyika.

Passing along the stomachic portion of the alimentary tube in Nautilus, we find that besides the diverticulum of the so-called gizzard there is another (fig. 2, *sp. c*). arising in conjunction with two con-

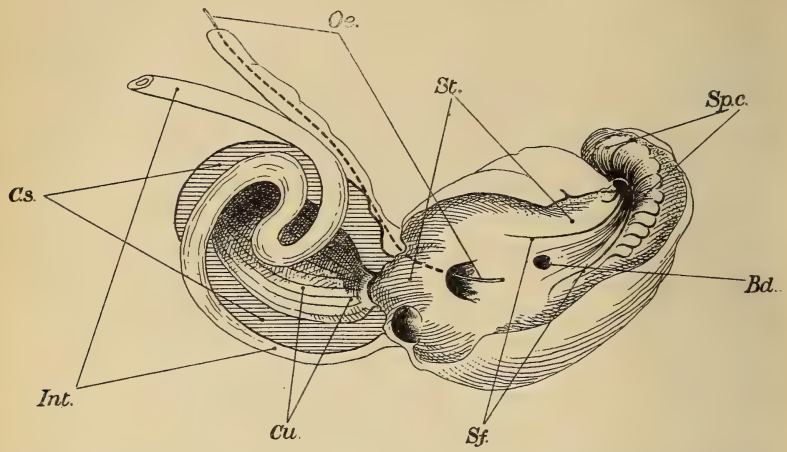


FIG. 1.—Gastric apparatus of *Nassopsis nassa*, dorsal aspect. *Oe.*, œsophagus. *C.s.*, cut-edge of wall of style-sac. *Cu.*, its cuticular lining. *St.*, stomach proper. *Sf.*, the two conspicuous folds leading into the spiral cæcum, *Sp. c.* *Bd.*, aperture of "bile duct." *Int.*, intestine.

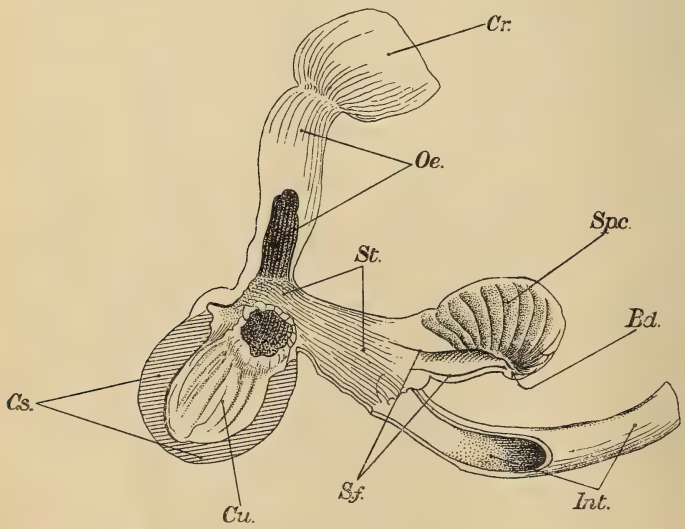


FIG. 2.—Dissection of gastric apparatus of *Nautilus pompilius*, dorsal aspect. *Bd* arrow indicating position of aperture of "bile duct." *Cr.*, so-called crop. Other letters as in Fig. 1.

spicuous folds (*s.f.*) which lead into a convoluted valvular arrangement, which projects beyond the parietes of the gastric tube. Between these folds the duct of the immense liver (*b.d.*) of Nautilus opens, just as the duct opens between the folds leading into the spiral cæcum of Nassopsis or Pleurotomaria among the Gastropods (compare figs. 1 and 2). Exactly the same results are obtained, but in even a more striking manner, if we compare the stomachic apparatus of Spirula (fig. 4) with that of Nassopsis.* In this case the stomach proper is far better developed than in Nautilus or Sepia (fig. 3, *st.*), and the correspondence between the different parts in the Cephalopod and the architænoglossan Gastropod is at once clear and striking.

From these observations it would appear that if we take Nassopsis as a Gastropod, and either Spirula or Nautilus as a Cephalopod, there is nothing incomparable in the stomachic apparatus of these widely divergent molluscan forms; on the contrary, they are exactly comparable in all their main features, and it would consequently appear to follow, firstly, that in both cases we are dealing with an extremely primitive type of organisation, and, secondly, that Nautilus among the Cephalopods and Nassopsis among the Gastropods retain a type of gastric apparatus which must have been possessed by the common ancestors of both.†

The different kinds of modification which this primitive gastric apparatus has undergone in the more specialised molluscan groups may be exemplified by what is found in the following types:—In Nassopsis, Spirula, Nautilus, and the Cephalopods generally, there is both a style-sac and a cæcum. In the Prosobranchiate Gastropod Paramelania there is a style-sac and a rudimentary cæcum. In *Turritella communis* there is a style-sac, and only a portion of a cæcum.

* In comparing the stomach of Spirula with other forms, we have been able to refer to the late Professor Huxley's unpublished notes and drawings relating to this and other Cephalopods preserved in the Roy. Coll. of Science, and it is most interesting to find that of all the Cephalopods examined the gastric apparatus in Spirula shows the closest similarity to that of Nassopsis and Limnotrochus among the Gastropods. Huxley always inclined to the belief that Spirula is the living representative of the Belemnites, and the above comparisons would certainly seem to show that it retains a very primitive alimentary canal.

† It is worthy of mention that the forms of Tænioglossan Prosobranchiata, which retain both the style-sac and the cæcum, if conchological deductions can be trusted, are among the very oldest of fossil forms. The Capulidæ, to which Limnotrochus and Chytra are allied, extend back into the Cambrian, and the same may be said of the Pyramidelidæ, which appear to be the ancient representatives of the Turritellidæ of the present day. Nassopsis, on the other hand, is more closely related to forms which, like Ampullaria, unite the characters of the Tænio- and Rhipidoglossate types. All the Cephalopods are much later in origin, and it would appear, as so often happens, that they retain an archaic character which the living representatives of the older diatocardiate Prosobranchs, such as Pleurotomaria, have partially lost.

In Tanganyicia the valvular folds of the cæcum are still more reduced. In Strombus and Pteroceras there is a style-sac, but nothing com-

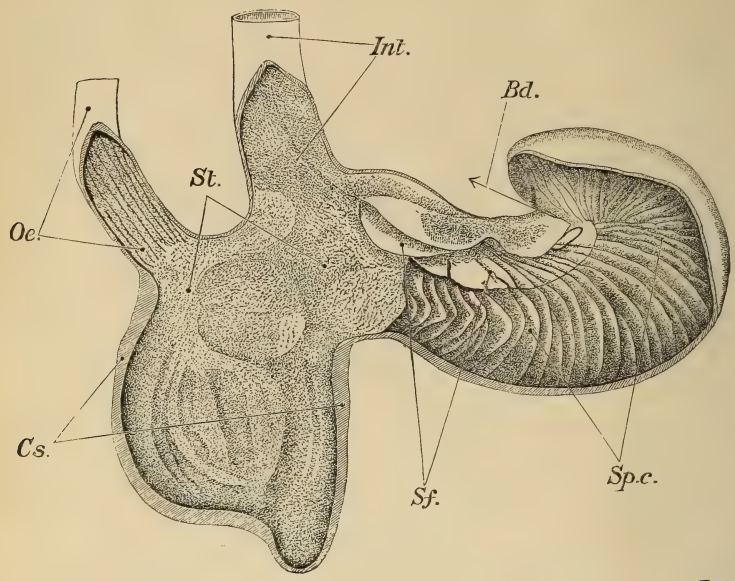


FIG. 3.—Dissection of gastric apparatus of *Sepia officinalis*, dorsal aspect. Letters as in figs. 1 and 2.

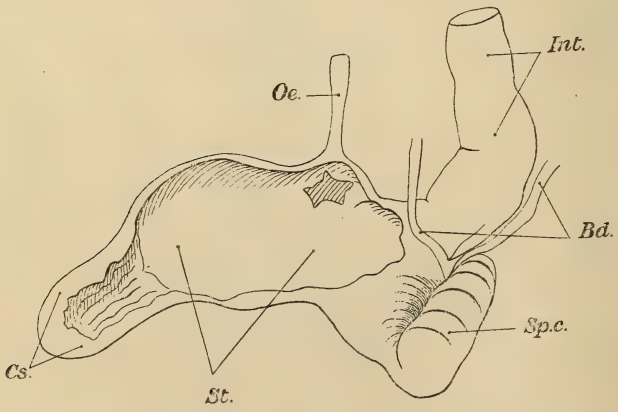


FIG. 4.—Gastric apparatus of Spirula, dorsal aspect. After Huxley. Letters as before.

parable to a cæcum; while in forms like Littorina and Vivipara there is neither cæcum nor sac. In the Lamellibranchiata there is often a style-sac, sometimes no trace of this, and there is no spiral cæcum.

Lastly, in the rhipidoglossate Gastropods, there is a cæcum but apparently no sac.

In conclusion, it would therefore appear that the so-called gizzard of Spirula and Nautilus, and consequently the similar organ which appears in other Cephalopods, is in reality the homologue of the style-sac of the Gastropods and the Lamellibranchiata. That in the Cephalopods only that portion of the gastric tube which lies between the orifice of the œsophagus and the intestine is the true stomach and consequently comparable to the true stomach of the Gastropods, while, lastly, the valvular diverticulum in relation to the "bile duct" in the Cephalopods is unquestionably the homologue of the similarly related and so-called spiral cæcum which projects from the true stomach of the Gastropods.

"Coefficients of the Cubical Expansion of Ice, Hydrated Salts, Solid Carbonic Acid, and other Substances at Low Temperatures." By JAMES DEWAR, M.A., D.Sc., LL.D., Jacksonian Professor, Cambridge University, and Fullerian Professor, Royal Institution, London. Received April 16,—Read May 1, 1902.

The apparent specific gravities of boiling liquid oxygen which resulted from weighing in the liquid a series of metals and other substances were given in a lecture entitled "New Researches on Liquid Air," printed in the Royal Institution 'Proceedings' for 1896. For instance, silver, calc spar, rock crystal, and iodide of silver gave the respective apparent densities 1·1278, 1·1352, 1·1316, and 1·1372. On correcting the weight of liquid displaced by each substance for contraction to – 182°·6—by calculating a Fizeau mean coefficient of expansion for the range of temperature employed, on the assumption that the parabolic formula might be legitimately extended to low temperatures,—it was found that the real density of liquid oxygen so deduced for all the bodies used was, as a mean, 1·137.

The determination of the densities of substances at the temperature of the boiling-point of oxygen,—and hence of their mean coefficients of expansion between that temperature and ordinary temperatures,—opens out a very large field of investigation, from which, if a sufficiently large number of observations were available, valuable deductions might be drawn. On account, however, of the expense and trouble of producing quantities of liquid oxygen, its use for this purpose is not likely to become general, although, when available, it is the easiest body to use in conducting such experiments, especially when the vacuum vessel containing it is immersed in a larger vessel containing the same fluid or well evaporated air. The ease with which liquid

air can now be obtained in many laboratories suggests that its application to work of this kind would in some cases be a convenience, and the present investigation was undertaken with the desire of ascertaining what accuracy could be attained, and how the method could be applied to inorganic or organic substances which occur in the form of fine crystals. The use of a mixture of varying composition and density like liquid air necessitates a determination of its density with accuracy and rapidity before and during the course of the experiments. For this purpose, in the experiments about to be detailed, the liquid air that had been allowed to evaporate for twenty-four hours in advance was used in large silver-coated vacuum vessels of some 3 litres capacity. In order to ascertain the density of the liquid, a polished silver ball, which had been weighed once for all in liquid oxygen, was weighed in the sample of liquid air, and from the relative weights thus found the density of the liquid air could be approximately determined, assuming that of liquid oxygen to be 1·137.* To prevent any disturbing ebullition in the liquid-air flask in which the weighings took place, and to reduce the rate of its evaporation to a minimum during the course of an experiment, the substance to be used was previously cooled in a supplementary vessel containing liquid air and then transferred to the large flask. To avoid as far as possible the formation of cracks in the bodies during the process of immersion in the liquid air, it was found advisable to cool them slowly in the air of the vacuum flask first, and then to lower them into the liquid. In this way, with proper care and attention, results were obtained comparable in accuracy with the density taken in liquid oxygen. Substances like solid carbonic acid and ice were weighed in the cool, gaseous air of the vacuum vessel, and their weights subsequently corrected for buoyancy. The temperature of the densest and lightest samples of liquid air were ascertained by the hydrogen thermometer, and that of the others deduced by graphic interpolation. As the entire range of temperature through which the bodies were cooled amounted to about 200°, a degree or two up or down has no real influence on the results; the extreme range of temperature in the air samples was from 83°·8 to 86°·1 Abs.

When the body to be examined was a salt, it was employed in the form of a compressed block. One experiment was, however, made in a section of a large crystal of chrome alum. The salt, previously reduced to a fine powder, was moistened with water and compressed in a cylindrical steel mould under great hydraulic pressure. During compression the saturated salt solution drained away, and finally a cylindrical block of some 50 grammes of the salt was obtained free from porosity and hard enough to allow its surface to be polished. In

* As the correction due to the contraction of the silver ball between the temperature of boiling oxygen and that of the air sample is small, it may be neglected.

this form salts and other materials similarly treated are especially adapted for accurate specific gravity determinations. After such treatment it was found that all the mechanically attached water was got rid of in the case of hydrated salts, and also in such as did not combine with water. In order to get cylindrical blocks of the salts showing no porosity the presence of water, or rather the saturated salt solution, was found to be essential during the application of pressure. In the same way it was found to be an advantage in compressing such a substance as solid carbonic acid, to moisten it with a fluid like ether before applying the hydraulic pressure.

Recalling the work of Playfair and Joule,* which originated in a suggestion of Dalton's that the volume of a hydrated salt in solution was simply the volume of the water of crystallisation, ice and some hydrated salts were selected, as well as some other bodies whose coefficients of expansion they had determined. Substances of special interest were included in the list, like mercury, sulphur, iodine, and solid carbonic acid, the latter being particularly important as an example of a solidified gas.

In the further conduct of an experiment the observations made on a substance were three, namely: (A) the weight in grammes of the substance and suspending platinum wire, either in air of about 17° C. temperature or in the gaseous air in the flask containing the liquid air; (B) the weight in grammes of the body and wire when immersed in the liquid air; and (C) the weight in grammes of the suspending platinum wire in ordinary (17°) air. This wire was bent into the form of a small cage, to hold the compressed cylinder or portion of the substance to be examined. (C) was measured once for all for each cage, and in the different cases varied from about 0·2 to 0·3 gramme. The relative portion of wire not immersed in liquid air was so small that no appreciable error will be made in (B) by considering that the whole of the wire was immersed. Further, no allowance is made for the (17°) air displaced by the weights themselves. The observed weights A, B, C are therefore connected with the weights and specific gravities of the substances concerned by the very approximate relations

$$A = W + w - \frac{W}{S}d - \frac{w}{s}d,$$

$$B = W + w - \frac{W}{S}D - \frac{w}{s}D,$$

$$C = \quad w \quad - \frac{w}{s}d,$$

* "Researches on Atomic Volume and Specific Gravity," 'Chem. Soc. Jour.,' vol. 1, 121.

where W = true weight of substance examined,
w = true weight of platinum wire,
S = specific gravity of substance required,
D = specific gravity of liquid air, when B was observed,
d = specific gravity of gaseous air when A was observed,
s = specific gravity of platinum.

From these relations we derive

$$S = \frac{A - C - B\frac{d}{D} + C\frac{d}{D}\frac{s-D}{s-d}}{A - B - C\frac{D-d}{s-d}} D,$$

instead of which we may write

$$S = \frac{A-C}{A-B} D \times \text{correcting factor.}$$

The correcting factor is

$$\frac{1 - \frac{d}{D}\frac{B - C\frac{s-D}{s-d}}{A-C}}{1 - \frac{D-d}{s-d}\frac{C}{A-B}},$$

which in the present circumstances differs from unity only by a few units on 10,000.

In the subjoined Table I are given the values of A – C, A – B and D for each observation.

In the case of substances of less density than liquid air, a polished copper ball weighing about 38 grammes was used as a sinker. The details of the ice experiments, three in number, are given in Table II.

Two experiments were made on compressed cylinders of solid carbonic acid. In the first of these the carbonic acid was compressed dry, in the second, after a few drops of ether were added. The specific gravities of solid mercury, iodine, and sulphur were also determined in liquid air. The iodine was in the form of a compressed cylinder, but the sulphur was a piece of a crystalline mass of native origin.

The specific gravity of the actual portion of the substance weighed in the liquid air was, with one or two exceptions, determined also at the temperature of the laboratory, about 17° C. From the two sets of observations the value of the mean coefficient of cubical expansion between 17° C., and the temperature of liquid air, was calculated.

In calculating coefficients of expansion, various forms may be given to the formula employed, and correspondingly different results may be obtained from the same set of observations. For short ranges of temperature these results are practically identical, but this no longer holds for a range of temperature such as we have in these experiments.

All that is possible in the present instance is to adopt a linear formula. The usual formula is $v_T = v_0 (1 + \alpha T)$, where the value v_0 at 0° C. becomes v_T at T° C. when α is the coefficient of expansion. If we use densities (d) instead of volumes (v) this formula becomes

$$d_0 = d_T(1+\alpha T), \qquad \text{or} \quad \alpha = \frac{d_0 - d_T}{T d_T}; \qquad \alpha = 0{\cdot}000538.$$

Another formula, when T and T′ are the temperatures dealt with, is

$$d_T = d_{T'}\{1+\alpha(T'-T)\}, \quad \text{or} \quad \alpha = \frac{d_T - d_{T'}}{(T'-T)d_{T'}}; \quad \alpha = 0{\cdot}000595.$$

Again

$$d_{T'} = d_T\{1-\alpha(T'-T)\}, \quad \text{or} \quad \alpha = \frac{d_T - d_{T'}}{(T'-T)d_T}; \quad \alpha = 0{\cdot}000558.$$

Also we may choose a mean formula

$$\alpha = \frac{d_T - d_{T'}}{(T'-T)\dfrac{d_T + d_{T'}}{2}}; \quad \alpha = 0{\cdot}000576.$$

The differences in the results of applying these formulæ are shown in the numerical values attached to each, which are calculated from the first experiment on solid carbonic acid in Table III, coupled with the specific gravity 1·53 of the solid at −78° C.

Perhaps as a matter of general convenience, the first of these formulæ is the best; however, the second was chosen to conform with the old work of Playfair and Joule, and it is the results of this formula which appear in the table.

The temperature range is taken from about −186° C. to 17° C., unless otherwise stated.

Ice.—In determining the density at the temperature of liquid air of pieces of clear ice cut from large blocks, both the silver and copper balls already referred to were used as indicated. The true weight *in vacuo* of the silver ball was 132·2855 grammes, and that of the copper ball was 38·0802 grammes. The observations and results are given in Table II. The mean of the three densities at −188°·7 C. is 0·92999.

Recently Vincent* has redetermined the density of artificial ice at the freezing-point, and also its coefficient of expansion. He finds the density to be 0·916, or from his tabulated results 0·91599. Playfair and Joule find the mean of the densities given by eight observers previous to them, to be 0·919, and they themselves get 0·9184; Bunsen found it to be 0·9167. If we take this most recent determination, namely, 0·91599 at 0°, and 0·92999 at −188°·7, and use the formula

$$d_0 = d_T(1+\alpha T)$$

we get $\alpha = 0{\cdot}00008099$.

* 'Roy. Soc. Proc.,' 1901.

Vincent refers to "only one" estimate for natural ice, namely, 0·0001125, adding that "the mean of three available results for artificial ice is 0·000160"; finally, he gives the mean of four determinations of his own, namely, 0·000152. Apparently then, we may take 0·0001551 as the mean coefficient of expansion of ice between 0° and (say) −20° C. Thus the mean coefficient of expansion between 0° and −188° C. is about half of that between 0° and −20° C. The mean coefficient of expansion of water in passing from 4° to −10° is −0·000362, and from 4° to 40° C. it is 0·0002155. Hence the mean coefficient of expansion of ice between 0° and −188° C., is about one-fourth of that of water between 0° and −10° C., and half of that between 4° and 100° C.

If we had the densities of ice at still lower temperatures, the values of the coefficient of expansion thence determined would, we have every reason to believe, be less than what we have found. We shall therefore not be overstraining the argument if we use the value just found to determine an upper limit to the density of ice at the absolute zero. The result is 0·9368, corresponding to a specific volume 1·0675. Now the lowest density of water, namely, at the boiling-point, is 0·9586 (corresponding to specific volume 1·0432), so that ice can never be cooled low enough to reduce its volume to that of the liquid taken at any temperature under one atmosphere pressure. In other words, ice molecules can never be so closely packed by thermal contraction as the water molecules are in the liquid condition, or the volume of ice at the absolute zero is not the minimum volume of the water molecules. It has been observed by Professor Poynting* that if we suppose water could be cooled without freezing, then taking Brunner's coefficient for ice, and Hallstrom's formula for the volume of water at temperatures below 4° C., it follows that ice and water would have the same specific volume at some temperature between −120° and −130°; applying the ordinary thermodynamic relation, then no change of state between ice and water could be brought about below this temperature. On the other hand, Clausius† has shown that the latent heat of fusion of ice must be lowered with the temperature of fusion some 0·603 of a unit per degree. If such a decrement is assumed to be constant, then about −130° the latent heat of fluidity would vanish.‡ Baynes discusses the same subject,§ and arrives at the conclusion that at a temperature of −122°·8 C. and under a pressure of 16,632 atmospheres

* "Change of State, Solid, Liquid," 'Phil. Mag.,' 1881.

† 'Mechanical Theory of Heat,' p. 172 (1879).

‡ In my paper "On the Lowering of the Freezing-point of Water by Pressure," 'Roy. Soc. Proc.,' 1880, it was proved that up to 700 atmospheres the rate of fall was constant and equal to the theoretical value within the range of pressure if the difference between the specific volumes of ice and water remain constant; thence the latent heat of fusion must diminish just as Clausius had predicted.

§ 'Lessons on Thermodynamics,' p. 169 (1878).

there is no distinction between the solid and liquid forms of water. At temperatures below this limit no amount of pressure would transform ice into water. We are thus relieved from a difficulty that would follow but for this demonstration of Clausius, namely, that the application of enormous pressures to ice, even at temperatures below that of liquid hydrogen, might cause the transformation of ice into water.

Carbonic Acid.—Two experiments were made with this substance, the masses in each case being about 20 grammes. These were compressed cylinders; the former was compressed dry, while the latter was slightly moistened with ether. The data and results are given in Table III.

The density of solid carbonic acid at its boiling-point was formerly given as 1·5,* but the mean of my results at the time came to 1·53. Recently the same value has been found by Behn. Taking this value and 1·6267, the mean of the above results at $-188°·8$ C., and using the formula $d_T = d_{T'}\{1 + \alpha (T' - T)\}$ we get $\alpha = 0·0005704$.

This is a very large coefficient of expansion, being greater than that of any substance recorded in Table I, and comparable with that of sulphur between 80° and 100°, which, according to Kopp, is 0·00062. The coefficient of liquid carbonic acid at its melting-point taken from the recent observations of Behn† is 0·002989, so that the rate of expansion of the liquid at its smallest value is very nearly five times that of the solid.

Solid Mercury.—One experiment was made with solid mercury, of which the details are given in Table I.

Mallet determined with great accuracy the density of solid mercury at $-38°·85$, his result being 14·193; coupling this with the density found for the liquid-air temperature, we find the value of the coefficient of expansion between the melting-point and $-189°$ C. is 0·0000887. For fluid mercury above 0° C. the mean value is about 0·000182, so that in the solid state this coefficient is about half of that in the fluid state.

Notes on the Results of Table I.

Sodium, extending down to low temperatures, has a coefficient about the same as that of mercury at the ordinary temperature. The coefficient for sulphur is about half of that between 0° and 100°, being 0·0002237, and that of iodine is not far removed from the value 0·000285 given for the solid at ordinary temperatures. The rate of expansion of liquid iodine is about three times this value Paraffin ought to have a value of 0·0004633 from Fizeau, but Rodwell's coefficient between 0° and 38° is 0·00035. The value found for

* See 'Proc. Roy. Inst.,' 1878, "The Liquefaction of Gases."

† 'Chem. Jour.,' 1901.

Table I.

	A−C.	A−B.	D.	d.†	d_{17}.	α 0·000.
Sulphate of aluminium (18)*.	45·338	26·961	1·0225	1·7194	1·6913	0811
Biborate of soda (10)	47·057	28·328	1·0405	1·7284	1·6937	1000
Chloride of calcium (6)	68·942	37·493	0·9347	1·7187	1·6775	1191
Chloride of magnesium (6)	36·663	22·438	0·9816	1·6039	1·5693	1072
Potash alum (24)	50·462	30·177	0·9816	1·6414	1·6144	0813
Chrome alum (24), large crystal	47·345	26·406	1·0226	1·8335	1 8199	0365
,,	40·147	23·665	1·0517	1·7842	1·7669	0478
Carbonate of soda (10)	55·675	34·863	0·9347	1·4926	1·4460	1563
Phosphate of soda (12)	57·085	35·848	0·9700	1·5446	1·5200	0787
Hyposulphate of soda (5)	53·121	28·836	0·9573	1·7635	1·7290	0969
Ferrocyanide of potassium (3)	55·914	29·115	0·9887	1·8988	1·8533	1195
Ferricyanide of potassium	68·902	35·961	0·9887	1·8944	1·8109	2244
Nitro-prusside of sodium (4)	49·614	28·526	0·9887	1·7196	1·6803	1138
Chloride of ammonium, sample i	34·539	21·032	0·9597	1·5757	1·5188	1820
,, sample ii	31·083	18·870	0·9597	1·5809	1·5216	1893
Oxalic acid (2)	23·614	13·293	0·9571	1·7024	1·6145	2663
Oxalate of methyl	56·465	37·760	1·0217	1·5278	1·4260	3482
Paraffin	15·675	16·031	0·9992	0·9770	0·9103	3567
Naphthalin	43·622	33·001	0·9347	1·2355	1·1589	3200
Chloral hydrate	53·321	26·436	0·9789	1·9744	1·9151	1482
Urea	42·811	32·326	1·0282	1·3617	1·3190	1579
Iodoform	140·012	32·381	1·0282	4·4459	4·1955	2930
Iodine	17·806	4·135	1·137	4·8943	4·6631	2510
Sulphur	25·499	13·813	1·137	2·0989	2·0522	1152
Mercury	77·719	5·135	0·9502	14·382	14·193‡	0887
Sodium	16·904	16·504	0·9818	1·0066	0·972	1865
Graphite (Cumberland)	22·221	9·986	0·9573	2·1302	2·0990	0733

* The figures ( ) refer to the number of molecules of water of crystallisation. † Density at the temperature of liquid air. ‡ At −38°·85.

naphthalin is about half that of the liquid near its melting-point, viz., 0·000785. If the liquid coefficient be taken at a corresponding temperature to that of the liquid carbonic acid when comparing it with the solid, then its value is 0·001213, or the coefficient would be now in the ratio of 4 to 1. The graphite calculated from Fizeau should be 0·0000929, which is greater than my value; but the samples were different. My two specimens of chloride of

Table II.—Ice.

Silver ball. Weight lost in (1) oxygen, (2) liq. air.	Density of liquid air.	Weight of ice and copper ball and wire.		Weight of copper ball and wire.		Weight of ice *in vacuo*.	Density of ice at −188°·7 C.
		Gaseous air at −188°·7 C.	Liquid air at −188°·7 C.	Air at 16° C.	Liquid air −188°·7 C.		
(1) 14·411 (2) 12·446	0·9820	47·080	33·430	38·075	33·933	9·0612	0·93032
(1) 14·409 (2) 12·042	0·9502	46·78	33·88	38·128	34·103	8·706	0·92646
(1) 14·407 (2) 12·597	0·9942	56·391	32·740	38·127	33·94	18·167	0·93318

Table III.—Solid Carbonic Acid.

Weight of solid CO_2.		Density of liquid air.	Weight *in vacuo*.	Density at at −188°·8 C.
Gaseous air at −188°·8 C.	Liquid air at −188°·8 C.			
19·865 22·55	7·905 9·37	0·9832 0·9502	19·919 22·61	1·6308 1·6226

ammonium gave nearly the same value, and the result is in agreement with that found by Playfair and Joule, viz., 0·000191. If a Fizeau coefficient for this salt is calculated, the value is 0·0000761, which in this case is far too small. The coefficient found for oxalic acid is again only a little smaller than that given by Playfair and Joule, viz., 0·0002748. As regards the hydrated salts, phosphate of soda, hyposulphate of soda, and chloride of calcium, having the respective values 0·0001384, 0·0001516, and 0·0006887, as found by Kopp, the low tem-

perature coefficients are much smaller in each case. With the exception of carbonate of soda and chrome alum, all the other hydrated salts have a coefficient of expansion not differing greatly from that of ice at low temperatures. Generally, the densities of the compressed blocks of different bodies agreed well with the results of other observers, but my potash alum had only a density of 1·614, whereas Playfair and Joule give 1·731. It will be noted that iodoform is a highly expansive body like iodine, and that oxalate of methyl has nearly as great a coefficient as paraffin, which is one of the most expansive solids. The correcting factor was used for paraffin, naphthalin, chloral hydrate, iodoform, and sodium. All weights are in grammes.

It will be possible by cooling the moulds with liquid air during the process of hydraulic compression, to produce cylindrical blocks of solid bodies of lower melting-points than any given in this investigation, such as alcohol, ether, nitrous oxide, ammonia, chlorine, &c., and to ascertain their coefficients of expansion in the solid state between the individual melting-points and the boiling-point of liquid air.

This method, which works well with liquid oxygen or air, fails when applied to liquid hydrogen, as the density of the liquid is too small (apart from other difficulties) to give accurate values of the weights of fluid displaced. For temperatures about 20° absolute, recourse must be had to measurements of the coefficient of linear expansion, and such observations could only be applied with ease to metallic bodies and alloys.

"Report of Absolute Magnetic Observations at the Valencia Observatory (Cahirciveen, co. Kerry), 1899, 1900, and 1901." By J. E. Cullum. Communicated by The Earl of Rosse, F.R.S. Received April 12,—Read April 24, 1901.

As far back as 1887 the late Professor FitzGerald, of Trinity College, Dublin, expressed a desire for a series of magnetic observations to be made in Ireland, when the writer offered to undertake the work if he was provided with instruments, and obtained the consent of the Meteorological Council. This was done, and an old pattern unifilar by Jones, with a dip circle by Barrow, being loaned by Trinity College, experimental observations were commenced in a wooden hut erected in the Observatory grounds in Valencia Island, with funds supplied by the Royal Irish Academy. The result of the first few years of this series was published in the 'Proceedings of the Royal Irish Academy,' and the observations were continued until the end of 1896. The observatory was removed in 1892 to Cahirciveen, about 4 miles eastward.

Seeing that no use was being made of these observations, and Trinity College requiring their instruments, Professor FitzGerald came to the conclusion to terminate the work, and the observations accordingly ceased. Soon after enquiries were made for them, and very strong expression of their value given, with a desire for their continuance. Acting on such weighty opinion, he determined to put the observations on a business and permanent basis by obtaining a set of modern instruments, and a fund to endow the work. Enough was collected in Ireland to ensure the observer a small annual honorarium.

With a new set of instruments, examined at Kew Observatory, the present series of observations was commenced in January, 1899, in the original wooden observing house erected in the field north of the Observatory, 90 yards from that building. A railway runs east and west about 50 yards north of the hut, and five light trains pass each way daily, but these do not appear to materially affect the magnets whilst observing. The observing house being copper-fastened, with no iron down-pipe, this is the only magnetic interference likely to affect the results.

In determining the position of the hut, consideration was given to obtaining a satisfactory fixed mark, visible from the observing pillar, and the down-pipe of a school house about 1 mile north of the Observatory was decided upon, as being the property of the Board of Works and likely to be permanent. Not having the means of obtaining sufficiently accurate time for a sun's transit, the orientation of this mark was determined by taking three series of observations of equal altitudes of the sun by means of a transit theodolite lent by Kew Observatory for the purpose; the mean of these was taken, and the value 19° 46′ 0″ W. used in the reduction of the declination observations.

The observatory is situated on an arm of Valencia Harbour, co. Kerry, lat. 51° 56′ N., long. 10° 15′ W., and is consequently about the most western part of the United Kingdom.

The unifilar is by Dover, No. 139, and, as before stated, was verified at Kew Observatory, where the constants of the magnet were also determined, and tables of corrections supplied in the usual manner.

The dip circle is also by Dover, No. 118; it was also examined and passed at Kew.

For the convenience of observing, the two middle days of each month have been adhered to throughout; the declination being observed about 10 A.M. Greenwich (local time 41 mins. later), the actual mean time for the three years being 10.9 A.M.

The mean time of the inclination observations is for the morning 11.45 A.M., and for the afternoon 1.25 P.M., these being made on succeeding days with two needles, which give unusually consistent results, the mean of each of the three years showing an excess in No. 1

of only 0′·2. The meridian of the circle is determined in January of each year, the value so obtained being used throughout the current year.

The intensity observations are made in the usual manner of vibrations and deflections, the same magnet being used as in the declination observations. Two observations are made on the same day, one before noon (11 A.M.), and the other afternoon (3 P.M.), so that the mean time of the two observations used in calculating the forces is 1 P.M., which very nearly agrees with the mean time for the inclination, with which they are combined in the formulæ (V.F. = H.F. × tan inclin.) and (T.F. = H.F. × sec. inclin.). The strength of the magnet considerably diminished during the first year, which may be attributed to its want of age; the magnetic moment (m) falling in 1899 from 770 to 728 units; in 1900 to 714; and in 1901 to 700, a decrease which will probably become less each year. The value of P (distribution of magnetism) in the reductions is calculated for the yearly series, in 1899 being 8·2; 1900, 7·0; and in 1901, 7·5.

Below are given monthly values of each element for the three years.

Year.	Month.	Declination.	Inclination.	Intensity.	Vertical F.	Total F.
		° ′	° ′			
1899	Jan....	21 39·1	68 34·9	0·17739	0·45220	0·48548
	Feb....	40·8	35·3	·17715	·45176	·48525
	March .	33·8	34·2	·17734	·45392	·48538
	April ..	32·3	34·9	·17741	·45227	·48571
	May ...	35·6	35·2	·17732	·45216	·48568
	June...	33·5	32·0	·17741	·45115	·48479
	July...	34·6	33·5	·17742	·45175	·48534
	Aug. ..	31·7	31·5	·17737	·45086	·48450
	Sept. ..	37·2	31·8	·17736	·45095	·48459
	Oct. ...	32·8	32·1	·17736	·45107	·48469
	Nov. ..	33·7	30·4	·17747	·45068	·48436
	Dec....	35·4	29·7	·17768	·45095	·48469
	Means .	21 35·0	68 33·0	0·17739	0·45164	0·48504

Year.	Month.	Declination.	Inclination.	Intensity.	Vertical F.	Total F.
		° ′	° ′			
1900	Jan. . . .	21 34·8	68 30·9	0·17738	0·45028	0·48435
	Feb. . . .	29·7	31·1	·17758	·45119	·48489
	March .	29·1	32·1	·17744	·45122	·48486
	April . .	25·9	30·3	·17754	·45071	·48444
	May . . .	30·0	30·6	·17765	·45138	·48509
	June . .	29·7	28·7	·17751	·45025	·48397
	July . . .	29·8	30·5	·17763	·45133	·48503
	Aug. . .	31·4	27·4	·17776	·45011	·48397
	Sept. . .	30·1	29·3	·17781	·45101	·48480
	Oct. . . .	29·0	27·8	·17770	·45035	·48414
	Nov. . .	29·6	27·9	·17782	·45065	·48446
	Dec. . . .	31·3	28·5	·17796	·45139	·48521
	Means .	21 30·0	68 29·6	0·17765	0·45082	0·48460

Year.	Month.	Declination.	Inclination.	Intensity.	Vertical F.	Total F.
		° ′	° ′			
1901	Jan. . . .	21 29·0	68 27·5	0·17792	0·45070	0·48456
	Feb. . . .	30·2	27·7	·17792	·45079	·48463
	March .	25·9	27·7	·17790	·45074	·48459
	April . .	26·8	27·7	·17783	·45056	·48438
	May . . .	26·5	26·9	·17804	·45079	·48469
	June. . .	26·6	27·2	·17797	·45072	·48460
	July . . .	26·4	25·3	·17803	·45014	·48407
	Aug. . .	28·7	25·4	·17803	·45018	·48411
	Sept. . .	29·4	24·3	·17799	·44967	·48362
	Oct. . . .	28·1	26·5	·17811	·45084	·48474
	Nov. . .	27·3	25·1	·17812	·45020	·48424
	Dec. . . .	27·4	24·1	·17827	·45030	·48431
	Means. .	21 27·7	68 26·3	0·17801	0·45048	0·48438

For the purpose of comparison, the published values of Kew and Falmouth are also given :—

Kew.

Year.	Month.	Declination.	Inclination.	Intensity.	Vertical F.	Total F.
		° ′	° ′			
1899		16 57·1	67 14·7	0·18393	0·43852	—
1900		16 52·7	67 11·8	·18428	·43831	—

Falmouth.

1899		18 32·7	66 48·7	0·18663	0·43569	—
1900		18 29·1	66 45·2	·18689	·43507	—

"Microscopic Effects of Stress on Platinum." By THOMAS ANDREWS, F.R.S., F.C.S., and CHARLES REGINALD ANDREWS. Received April 23,—Read May 15, 1902.

[PLATE 4.]

The microscopic effects of stress on platinum do not appear to have been studied. An ingot of pure platinum was therefore prepared, and from this a portion was accurately machined in the form of a cube, 0·30 inch square, which was afterwards carefully microscopically polished, and then subjected to compressive stress in the testing machine.

Prior to the application of stress, and for comparative purposes, a polished face of the platinum cube was microscopically examined, but an even polished surface only was observed. A force producing a compression of 10 per cent. on the total height of the cube was then applied, and microscopic observations were taken at high magnifications of the effects of the stress on the micro-crystalline structure of the platinum cube. The particulars of the stress applied are given on Table I.

Table I.—Compressive Stress applied to Platinum Cube.

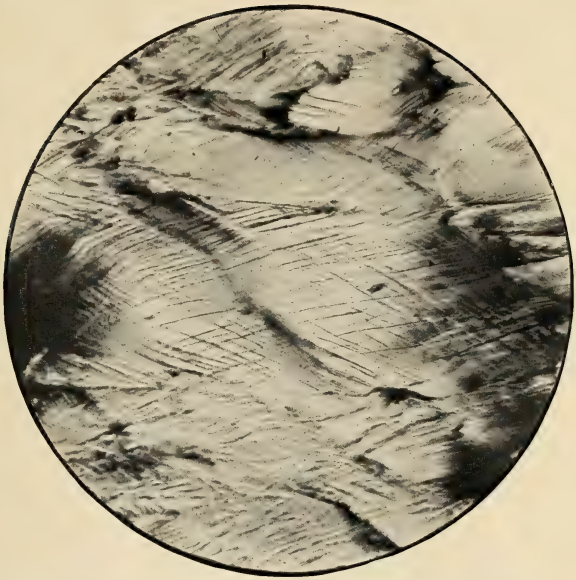
	Cross-section. Dimensions, inches.	Area. Square inches.	Height before test. Inches.	Stress required to compress sample 10 per cent. of its original height. Tons per square inch.
Platinum cube..	0·30 × 0·29	0·087	0·30	12·82

The polished side of the cube upon which the high-power microscopic examination was made was the one in line, or in parallel, with the direction of the compressive force. The results observed were very interesting, and confirm Professor Ewing's and Mr. Rosenhain's observations on the peculiar manner in which the crystalline structure of a mass of metal becomes altered when under stress.*

On comparing the appearance of the platinum after having been stressed with its appearance before the application of stress, it was found that many of the individual large or primary crystal grains forming the mass had, under the influence of the strain, developed innumerable fine "stress bands" or "slip bands," or indications of crystalline slip. The main lines of disruption were noticed in many instances to roughly approximate to an angle of about 45° to the

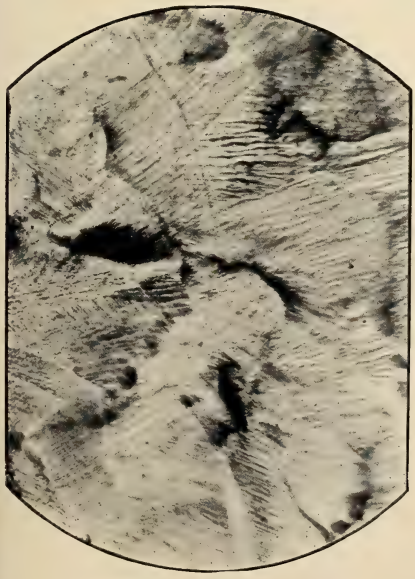
* Bakerian Lecture, 1899, 'Phil. Trans.,' A, vol. 193, 1900, p. 353.

FIG. 1.



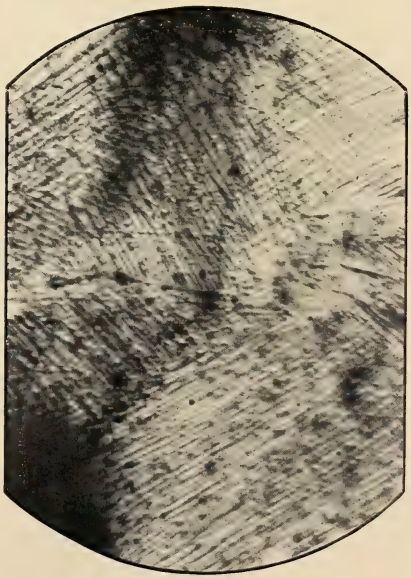
Microscopic Effects of Compression Stress on Platinum, showing Crystalline Slip, as seen in Section. Magnification 120 diameters. Vertical illumination. Metal compressed 10 per cent. Arrow indicates direction of compressive force.

FIG. 2.



Magnification 120 diameters.

FIG. 3.



Magnification 250 diameters.

Microscopic Effects of Compression Stress on Platinum, showing Crystalline Slip, as seen in Section. Vertical illumination. Metal compressed 10 per cent. Arrow indicates direction of compressive force.

line of the compressive force on the crystal sectional facets. Between these larger indications of crystalline slip were observed a number of extremely fine lines, indicative of the nature of the crystalline slip; many of these ran more or less in parallel lines in each individual primary crystal or crystal grain, the direction of the orientation (or line of the normal cleavage) of each primary crystal grain apparently influencing the direction of the "slip bands," as seen in section.

Owing to the varied orientation of the different crystals in the mass of the platinum,* the lines of cleavage as indicated by the minute "slip bands," were often seen at varied angles to the line of the straining force.

These experiments have also confirmed the observations of Professor Ewing and others, that stress alone, without etching, sometimes renders manifest the lines of intercrystalline junction of the large or primary crystal grains of a stressed metal, providing that the stress is of sufficient intensity. This will be seen on reference to the accompanying illustrations, Plate 4.

The general appearance of the disintegration of the large or primary crystal grains, produced by the pressure, on the pure platinum cube, was the apparent breaking up of the crystalline structure of the metallic mass, as seen in section, roughly diagonally to the line of the compressive force. The area enclosed by the main lines of disruption roughly approximating to the size of the large primary crystal grains. The distances between the extremely fine lines, or "slip bands," appeared roughly to coincide proportionately with the size of the secondary or most minute crystals forming the mass, the finer "slip bands" appearing to indicate the crystalline slip which had taken place along the facets of the smaller or secondary crystals. The direction, however, of the main lines of the crystalline disruption did not appear always to coincide with the intercrystalline facet junctions of the large or primary crystal grains. The lines of least resistance, or greatest crystalline slip, seemed chiefly to develop at an approximate angle of about 45 degrees to the pressure line, as previously mentioned; but the line of greatest weakness in the mass structure of the metal was not always at that angle with the line of the disruptive force.

The previous description generally and approximately indicates the effect observed, but the breaking up of the crystalline structure of the metal appeared to be subject to modifications according to varied internal or external conditions.

* See "Micro-crystalline Structure of Platinum," by Thomas Andrews, 'Roy. Soc. Proc.,' vol. 69, March 21, 1902, No. 457, p. 433; also "Microscopic Structure of Gold and Gold Alloys," by Thomas Andrews, 'Engineering,' September 30, October 28, December 9, 1898; also "Micro-metallography of Iron," by Thomas Andrews, 'Roy. Soc. Proc.,' vol. 58, 1895.

The authors hope that these experiments may prove of use in affording an indication of the comparative behaviour of this "ἀριστο-κρατία" or noblest metal platinum, with the behaviour of the constructive metals, copper, nickel, iron and steel, when under the influence of stress; and the experiments have also shown that the microscopic influences of stress in the heavy metal platinum are analogous to those which have been observed in metals of lower specific gravity.

DESCRIPTION OF PLATE.

Fig. 1.—Microscopic Effects of Compression Stress on Platinum, showing Crystalline Slip, as seen in Section. Magnification 120 diameters. Vertical illumination. Metal compressed 10 per cent. Arrow indicates direction of compressive force.

Fig. 2.—Microscopic Effects of Compression Stress on Platinum, showing Crystalline Slip, as seen in Section. Magnification 120 diameters. Vertical illumination. Metal compressed 10 per cent. Arrow indicates direction of compressive force.

Fig. 3.—Same as fig. 2, but magnified 250 diameters.

"A Note on the Recrystallisation of Platinum." By WALTER ROSENHAIN, B.A. (Cantab.), B.C.E. (Melbourne). Communicated by Professor EWING, F.R.S. Received May 1,—Read May 15, 1902.

In a recent paper* Professor Ewing and the present author have described phenomena of recrystallisation in a number of metals, such as lead, tin, zinc, and cadmium, at temperatures well below the melting points of those metals. I have recently observed phenomena which appear to me to be of a very similar nature in the case of platinum.

It is a well-known fact that a prolonged exposure to a high temperature renders platinum brittle, and that the surface of such platinum, when it has been exposed to flame, shows markings "resembling the appearance of galvanised iron." † This phenomenon has generally been ascribed to the action of carbon, and by one author specifically to the action of acetylene.† Having studied the phenomena closely with the aid of the microscope, I do not find this view entirely confirmed.

In the first place, on examining the surface of the "changed" platinum with the microscope, it is seen to show a pattern which is

* "On the Crystalline Structure of Metals," second paper, 'Phil. Trans.,' A, 1900, vol. 195, pp. 279—301.

† "The Action of the Acetylene Flame on Platinum," J. J. Redwood, 'Soc. Chem. Industry Journ.,' vol. 17, p. 1107; also 'Zeitschrift für Analyt. Chem.,' 1901, heft 6, p. 411.

characteristic of the *etched* surface of a crystalline metal. The micrographic appearance under oblique light is shown, magnified 30 diameters, in the photograph (fig. 1). This was taken from the surface

FIG. 1. Surface of a Platinum Crucible after prolonged heating. Oblique light. Magnification, 30 diameters.

of a platinum crucible which has been in continuous use and is frequently exposed for hours together to an ordinary blowpipe flame. A closer examination revealed a multitude of geometrical pits (Aetzfiguren), clearly demonstrating that the appearance is due to genuine metallic crystals which have been *etched* on the surface by some chemical agent. Two other experimental facts confirm this idea. The first is that if a piece of platinum showing the "changed" surface be exposed to the action of aqua-regia, the appearance is intensified and brightened. If, as has been suggested, the appearance were due to a superficial layer of a platinum-carbon compound from which the carbon had been driven off, leaving mere pseudomorphs of platinum, the etching action of the aqua-regia would have dimmed the appearance instead of brightening it. Another objection to the carbonisation theory is to be found in the fact that I have produced the effect on a *new* platinum crucible by prolonged heating in an oxygen injector furnace, where an oxidising atmosphere was being maintained.

The "changed" platinum is, moreover, very weak and brittle when hot, and on one occasion a crucible was torn whilst still red hot, but after being removed from the flame. The fracture was as crystalline

as that of "brittle" zinc, and the lines of fracture ran across the crystals revealed on the changed surface in such a way as to show that these crystals seen on the surface are not a thin superficial layer, but genuinely represent the entire structure of the metal.

I am therefore led to the conclusion that the action is simply one of recrystallisation. The metal in the state in which it reaches us in foil or crucibles, &c., is in a condition of severe strain, having been bent, drawn, rolled, &c., either in the cold or at temperatures far below its "annealing" temperature. This is supported by the fact that the platinum in its "unchanged" state shows a very minute structure characteristic of severely strained metals. The natural effect of exposure to a high temperature of metal in such a condition is to allow it to recrystallise, and this I conceive to be what occurs in the case of platinum. The brittleness of the "annealed" metal is not at all surprising, as the same phenomenon occurs with zinc and cadmium (see paper cited above). In the case of platinum "annealed" in a gas flame there is, however, a further action; simple annealing or recrystallisation, although it will completely alter the interior structure of a piece of metal, will not of itself alter the appearance of the surface even in microscopic detail. To develop a surface pattern corresponding to the changed internal structure the surface must be *etched after* the recrystallisation has taken place. The etching action is in this case undoubtedly due to the gases of the flame, and the temporary formation of a carbide may play a part in this process.

"On some Phenomena affecting the Transmission of Electric Waves over the Surface of the Sea and Earth." By Captain H. B. Jackson, R.N., F.R.S. Received May 1,—Read May 15, 1902.

In 1895, systematic experiments were commenced by me with a view of utilising the effect of Hertzian waves on imperfect electrical contacts, for naval signalling purposes.

I soon observed that some unexpected phenomena were deterrent factors in obtaining the necessary accuracy at all times, and with the most modern and improved instruments that we now possess, this is equally noticeable.

The results of some of the phenomena are described in this paper, with the conclusions that I have drawn as to their cause.

Some of the experiments described were specially conducted with the object of elucidating definite results on the subject. Other experiments carried out with a different object, and also ordinary practical signalling at various times, also gave the results described, without in

any way detracting from their value on account of the main object in view being at the time of a different nature.

One of the great difficulties in accurately comparing results, separated by even the shortest intervals of time in wireless telegraphy, is, undoubtedly, in having to depend on an imperfect electrical contact for the comparison, as even the best coherers, as they are now generally termed, cannot be depended upon to give absolutely similar consecutive indications, under similar conditions of excitation; and in the filings coherer, for instance, which I have chiefly used, the probability is infinitesimally small that the filings will rearrange themselves identically, in any two successive signals, taking into account the shaking to which they are subjected immediately following the reception of an excitation; though the practical results obtained with those of good manufacture might almost lead one to an opposite opinion, owing to the accuracy with which signals are generally recorded. Careful observation over an extended period has led me to the conclusion that the error arising from this cause in the experiments described does not exceed 5 per cent., *i.e.*, in any one hundred consecutive signs in a series of signals, ninety-five of them will be accurately recorded at the distance where the excitation is nearing the limits at which it will affect the coherer. At shorter distances, with moderately strong excitations, the error is much less, probably less than 1 per cent. Experiments conducted under favourable circumstances, with the object of obtaining the percentage of accuracy, have frequently confirmed this. The method used to carry this out was to select a long signal whose details were known, and count the errors in the number of signs composing the signal.

Though a possible error of 5 per cent. in any experiment may, in systematic comparative trials, or quantitative analysis, appear large, this error may be considered to be eliminated when the mean of a considerable number of excitations is taken, and the results judged by the general effect, which was whether or no excitations were received at a given distance for a considerable period of time. For instance: the experiments, as a rule, consisted in one station sending signals consecutively for (say) 2 minutes, at the rate of 100 signs a minute. If these signals were received correctly, or nearly so, the receiving station was considered to be within the limits at which signals could be received under those circumstances; if only 50 to 90 per cent. of the signs were received, it was considered at the extreme limit of signalling distance; if 20 to 50 per cent. were received, it was considered to be just outside the limits; if less than 20 per cent., or, as frequently occurred, no signs were received, it was considered to be beyond the limits of signalling. In the two latter cases, careful inquiry and examination of the records of the transmitting station were always made to prove that the signals had actually been sent at that time. At the con-

clusion of the 2 minutes, the receiving ship, by means of a short prearranged signal, acknowledged the signals, if received, and stated whether they were clear or broken, or if none had been received. To still further reduce any chance of error, two coherers, or, rather, two complete receivers, were generally used; they were adjusted to be of equal sensitiveness, or nearly so, and they were used alternately, for about 15 seconds each, or else together, *i.e.*, in parallel. Local excitation of these coherers was also frequently resorted to, as by this means, if the filings have by chance rested in an insensitive position, their excitation and shaking gives the possibility that they may rearrange themselves more sensitively.

The experiments recorded have, with few exceptions, been carried out under my own personal supervision at one of the stations, and the recording tapes of the receivers were invariably compared with the written notes and log of the transmitting stations, at the earliest opportunity afterwards.

The distances up to which I have carried out experiments, reach 140 nautical miles. In my early experiments, before wireless telegraphy had developed into its present stage, the distances were comparatively short, though the results were equally instructive. In the tables of records, in addition to stating the actual distances, the results are compared to a maximum distance of 100, which represents that obtained at sea under the same circumstances, but without the disturbing causes whose effects were under consideration at the time; so the percentage of loss of distance, under varying circumstances, may be easily compared.

The trials were all carried out under practical sea-going conditions, and though these may militate against the absolute accuracy obtainable in laboratory experiments, they may eliminate some errors which may affect the observations of Hertzian waves in a closed building, such as the reflection of the waves from the surrounding walls.

The instruments used were those constructed on what is generally known as the Marconi System, and were either supplied by his company or made to my design for the Admiralty; these types only differ in details.

As a rule, for transmitting signals, an aerial wire or wires were attached to one of two spark balls fitted to an induction coil, the other ball being earthed; when, however, my system of syntonic transmission was used, the connection of the aerial was not as above; this system is indicated by inserting (T) against the results. The induction coils employed have been those capable of giving a 10-inch spark between points in dry air. In all the experiments herein detailed, a jigger, *i.e.*, a small transformer, was used in the aerial wire of the receiving instruments. The aerial wires of all ships and stations were tuned as closely as possible to the same natural frequency of oscilla-

tion, and the jiggers were constructed to receive signals at the maximum distance corresponding to the power and frequency used at the transmitting station. The fundamental wave-length was that adopted in H.M.'s service, except when my syntonic system was used, in which case large variations in the wave-lengths were tried. Syntonic wireless telegraphy, however, is not considered in this paper.

The results, when tabulated, demonstrate the loss in distance that may be experienced when signalling by means of wireless telegraphy under varying conditions of the atmosphere, and the environment of the ship as regards land compared with the distance obtained under favourable conditions in the open sea. In fact, they show some of the causes which may affect the transmission of waves of electrical induction from an aerial wire over the surface of the globe. The causes are treated in the same order as I observed them in the practical work of wireless telegraphy, and the first case that I consider is the effect of intervening land on the distance at which signals can be recorded.

This effect, without any recorded exception, is to reduce the maximum distance of signalling from that recorded in the open sea by an amount depending upon the thickness, contour, height, and nature of the land. The curvature of the Earth and its effect on the waves is not taken into account, as at the distances covered in these experiments no effect has been noticed, though carefully watched for when signalling with stations high above the sea-level.

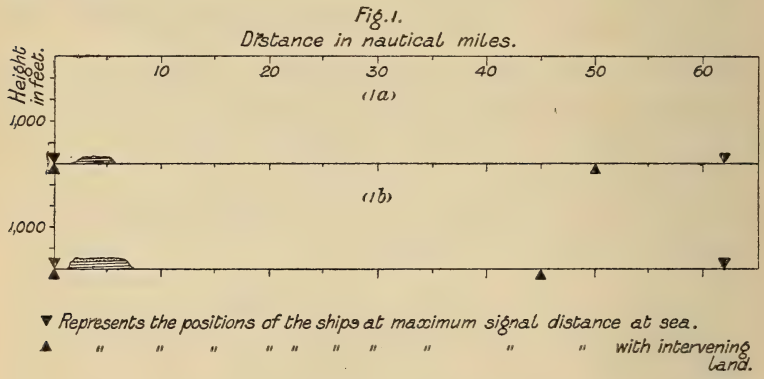
The effects of intervening land are shown for a few typical and well-authenticated cases, and where no other cause or known source of error existed, in the form of diagrams and tables. In the latter the heights of the mastheads or aerial wires are indicated, also the distance of the intervening land from the ship nearest to it, &c. The particulars of the land, the distances at which signals could just be clearly received, and the ratio of the distances at which signals were obtained over water under identical circumstances, and generally on the same day, are also inserted. The diagrams illustrate these details graphically, and show the general contour of the land and its strata. The vertical scale has been greatly exaggerated over the horizontal one, in order to present these data more clearly, and the ratio of the vertical to the horizontal scale is either 25 or $12\frac{1}{2}$, except in Nos. 4 and 5, where it is 32.

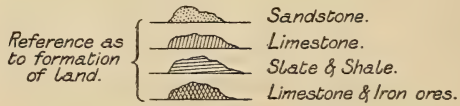
An examination of these results shows the marked difference between the effects due to the various natures of the intervening land.

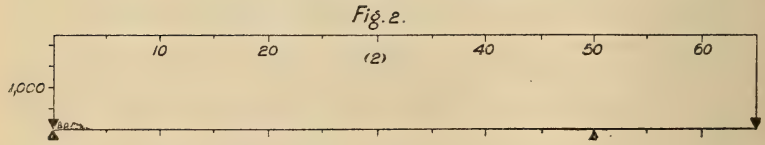
Summarising them for soft rocks, hard limestone, and limestone containing a large proportion of iron ores respectively, the percentage of maximum signalling distance through them compared to the open-sea distance is as follows :—

	Soft sandstone, shale, &c.	Hard limestone.	Iron ores.
Max. distance	81	68	Less than 40
Min. ,, ...	56	25	,, 23
Mean ,, ...	72	58	,, 32

Consider, firstly, the soft rocks :—The two maxima percentages of distance (81 and 80) are over rather low land of no great thickness; the minimum, 56 per cent., is over high land, half as thick again as in these cases.







Secondly, the limestone :—The maximum percentage (68) is over the thinnest layer recorded of limestone (3 *b*), the minimum (less than 25) is over a precipitous high mountain through which no signals could be passed at any distance, though they were obtained without difficulty over a low promontory of the same island and of the same formation, when both ships had moved to such positions as to bring the low instead of the high land between them (figs. 3*h* and 3*g*).

Reference Table for the above Figures.

Reference number.	Height of aerial wire above sea.	Distance from the land.	Particulars of the land.				Height of aerial wire above sea.	Maximum signal distance.		Percentage of maximum distance over land to over sea.
			Maximum height.	Total thickness.	Formation, strata, &c.	Surface.		At sea.	Over the land.	
	feet.	miles.	feet.	miles.			feet.	miles.	miles.	
1*a*	158	2	150	4	Shale	Pasture, wet	178	62	50	81
1*b*	158	$1\frac{1}{4}$	250	7	Sandstone and slate	do.	178	62	45	73
2	125	130 yards	200	$2\frac{1}{4}$	Porous sandstone	Buildings, dry	160	65	50	77

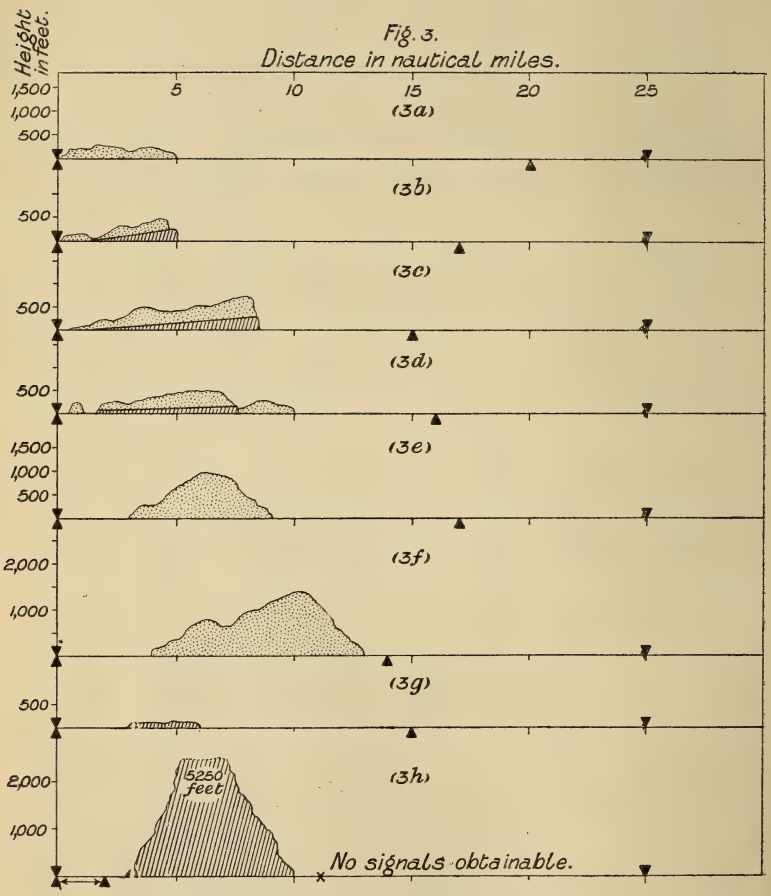
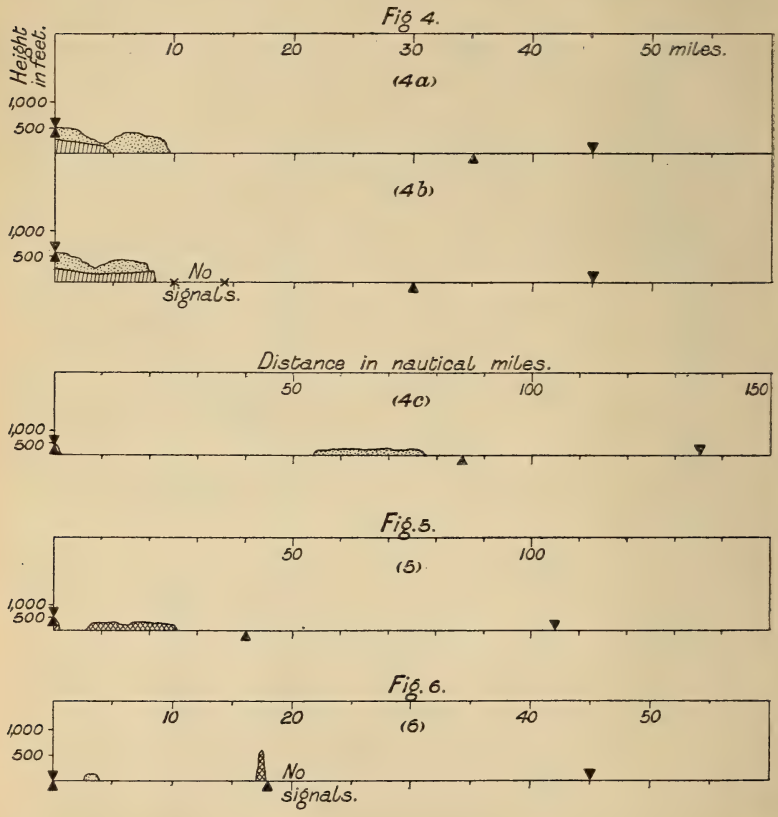


Fig. 3.
Distance in nautical miles.
Height in feet.
5
10
15
20
25
1,500
1,000
500
(3a)
(3b)
(3c)
(3d)
(3e)
2,000
(3f)
(3g)
(3h)
5250 feet
No signals obtainable.

Reference number.	Height of aerial wire above sea.	Distance from the land.	Particulars of the land.				Height of aerial wire above sea.	Maximum signal distance.		Percentage of maximum distance over land to over sea.
			Maximum height.	Total thickness.	Formation, strata, &c.	Surface.		At sea.	Over the land.	
	feet.		feet.	miles.			feet.	miles.	miles.	
3*a*	125	250 yds.	250	6	Porous coral sandstone..	Cultivated, wet	160	25	20	80
3*b*	—	220	500	6	Do. over limestone	do.	—	—	17	68
3*c*	—	500	700	8	do.	do.	—	—	15	60
3*d*	—	1000	500	7	do.	do.	—	—	16	64
3*e*	160	3 miles	1083	6	Gritstone and marl	Bare and dry, with scrub	125	—	17	68
3*f*	—	4	1400	9	do.	do.	—	—	14	56
3*g*	—	3	120	3	Semi-crystalline limestone	do.	—	—	15	60
3*h*	—	1 to 2½	5250	7		do.	—	—	No signals.	Very small.

Thirdly, the rocks containing iron ores :—In all these cases a greater loss of proportional distance is recorded than in the others—and it was exceptional to receive any signals at all—and the best result recorded in several trials was but 39 per cent. of the open sea distance (fig. 5).



The results shown in fig. 6 are the most conclusive that I have obtained in proving the screening effect of hard rocks containing iron ores on the passage of electric waves through land. The pinnacle of rock shown therein represents an extremely precipitous, narrow, but high promontory jutting out from the mainland and rising abruptly out of the sea, to which it is *steep to*, so that the ship could pass close to it in perfect safety at a distance of about 100 yards.

To ascertain the effect of this wedge-like obstruction, the ship was steered close to the land, and her position was carefully noted when signals ceased or commenced. These signals were being sent continuously from another vessel (distant 18 miles) during the whole period of the trials, the letter F (– – ——— –, in Morse Code) being

Reference number.	Height of aerial wire above sea.	Distance from the land.	Particulars of the land.				Height of aerial wire above sea.	Maximum signal distance.		Percentage of maximum distance over land to over sea.
			Maximum height.	Total thickness.	Formation, strata, &c.	Surface.		At sea.	Over the land.	
	feet.	miles.	feet.	miles.			feet.	miles.	miles.	
4*a*	160 + 500	On the land	500	9 over	Porous sandstone.....	Cultivated, wet.....	110	45	35	78 (T.)
4*b*	do.	—	600	8 over	Do., over limestone	do.	110	45	30, but none from 10 to 14	67 (T.)
4*c*	do.	—	400	22	Sandstone.....	do.	130	135	85 and over	Over 63
5	154 + 330	3	500	17	Limestone and iron ores..	do. wet and dry	125	105	40	39
6	125	100 yards	800	¼	" "	do. wet.....	110	45	None at 18	Less than 40 (T.)

made by her at the rate of twenty-five per minute by my syntonic transmitter.

The results showed that the signals ceased and commenced abruptly at the moment that the aerial wire passed the tangent from the transmitting ship to the edge of the cliff; the action was so abrupt, that, on one transit, the latter part of the long sign in the "F" was the first indication of signals that was received; and on another transit, in the opposite direction, the long of the "F" was the last sign received, the short being dropped; these were unusual results, as the signals generally die away gradually, the long signs breaking up, thus: (. . . . . .), and the shorts appearing as dots (.), before any signs are actually lost.

Another point that may now be considered, is the case shown in fig. 4*b*, when signals could not be exchanged when the ship was close under the land, but could be when clear of the land and in the same direction as before; the trial was repeated on several occasions for verification.

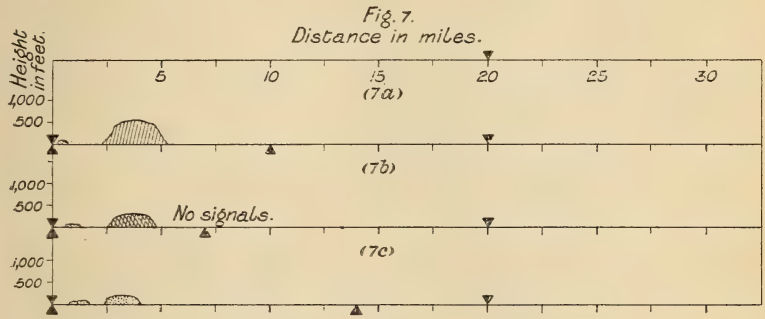
Possibly the case previously considered, fig. 6, is of the same class, as it is noteworthy, that when the ship was further off the promontory and also from the transmitting ship, though the two ships were still masked by high land of much greater thickness than before, a few stray signals were received occasionally, which evidently passed over, not round, and not through the land, as the ship was then in a land-locked bay.

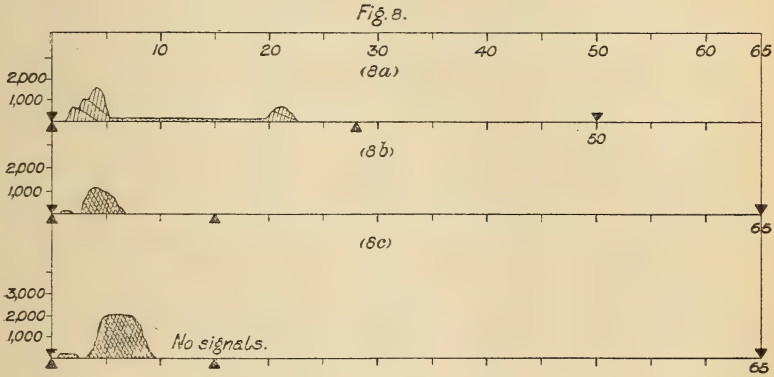
Referring now to figs. 3*e* and 3*f*, where the intervening land was both higher and thicker, and yet did not stop signals at longer proportional distances, it may be concluded that the waves of electrical induction, which must pass from ship to ship in order to record signals, may in certain cases pass through the land. Fig. 2 is a good example of this: one of the ships was lying alongside a perpendicular cliff of considerable height, and yet only experienced a loss of distance of about 12 per cent.

Fig. 8*a* gives a typical case of waves passing through valleys, and the results were so marked and so frequently repeated with different ships and on separate occasions that eventually the track of a vessel, proceeding at a known speed, could be roughly estimated, though distant 25 miles, by noting the intervals between the times when signals were lost and when received, and comparing these intervals with the time taken by the ship to cover the distances between the valleys, which were well delineated on the chart, and through which the waves could evidently wind their way with less obstruction than by any other route.

We have thus obtained evidence that the waves of electric induction may pass (1) through land, (2) over land, (3) round land, but that a large proportion of their energy is lost in doing so. (4) That the

screening effect of the land varies with its nature, and is greater for iron ores than for limestone alone, and that for this latter, it is greater than for soft rocks. No effects which could be attributed to interference of waves, due to reflection from a hilly background, have been recorded by me.





The next cause that I shall consider is that due to the varying conditions of the atmosphere; some of these conditions constitute a most serious obstacle to the effective transmission of electric waves over medium distances, and are, in consequence, a source of error likely to be encountered, and which cannot be foretold nor allowed for in wireless telegraphy.

As far as my experiences go, these effects are much less frequently noticed in temperate than in sub-tropical regions. In the Mediterranean Basin they seem to be particularly prevalent, and most persistent in summer and autumn.

Owing to their sudden advent and their equally sudden cessation, it is most difficult to carry out systematic or pre-arranged experiments, and I must therefore confine my remarks to general observations as to their effects in various parts of the Mediterranean Sea.

Reference number.	Height of aerial wire above sea.	Distance from the land.	Particulars of the land.				Height of aerial wire above sea.	Maximum signal distance.		Percentage of maximum signal distance over land to over sea.
			Maximum height.	Total thickness.	Formation, strata, &c.	Surface.		At sea.	Over the land.	
	feet.	miles.	feet.	miles.			feet.	miles.	miles.	
7*a*	85	$3\frac{1}{2}$	834	$2\frac{3}{4}$	Limestone.....	Scrub and wood. Wet	55	20	10	50 (T.)
7*b*	85	3	432	$1\frac{3}{4}$	Limestone and much iron ore	do. Dry	55	20	None at 7	Less than 35 (T.)
7*c*	85	3	260	$1\frac{1}{2}$	Sandstone.....	do.	55	20	14	70 (T.)
8*a*	125	1	1800	22 16 plain	Limestone. Valleys between hills	do. Wet and dry	130	50	28	56
8*b*	125	2	1200	4	Limestone and iron ores..	Bare. Wet and dry	160	65	15	23
8*c*	125	2	2060	6	do.	do.	160	65	None at 15	Less than 23

The first case is that due to the effects of lightning discharges, which may or may not be visible at the station where its effects are noticed. As a rule, with the instruments in normal adjustment, the effect of every discharge is to record a signal, the exceptions being very few.

The method adopted to observe this was to fit an electrical bell, worked by the receiving instruments, close to the observer, and at night observe the flashes and note if the bell rang.

For detailed observations, it was found more convenient to record the effects on the tape, and this was the method subsequently adopted. On the approach of the area of disturbance towards the ship, the first visible indication generally is—the recording of dots at intervals varying from a few minutes to a few seconds; secondly, the recording of three dots with a space between the first two, thus: (– – –) or *e i*, in the Morse Code, and this is the sign most frequently recorded by distant lightning; thirdly, the recording of dashes; the intervals between these then gradually decrease and merge into irregular signs, which have sometimes spelt words in the Morse Code; the effects generally die out more suddenly than they appear.

They are much more frequent in summer and autumn than in winter and spring—in the neighbourhood of high mountains than in the open sea—in southerly than in northerly winds (in the Mediterranean Sea)—in the front of a cyclonic disturbance of the atmosphere than in the rear, and with a falling barometer than with a rising one. In settled fine weather, if present, they reach their maxima between 8 and 10 P.M., and frequently last during the whole night, with a minimum of disturbance between 9 A.M. and 1 P.M.

The next cause which is intimately connected with the above, is the shorter distance at which signals can usually be received, when any electrical disturbances are present in the atmosphere, compared to the distance at which they can be received when none are present. The distance varies from about 30 to 80 per cent. compared with that obtained in fine clear weather. It does not in any way decrease with the increase of the number of lightning discharges which register their effect on the instruments, at any given time, but rather the reverse, the loss in distance generally preceding the first indications, on the instruments, of the approaching electrical disturbance.

A very marked case is given as an example: Two ships whose instruments were in perfect order, and whose sea-signalling distance was about 65 miles, opened their distance from each other on a fine, calm, bright day; when they were 22 miles apart, the signals died away, though there was no intervening land or other apparent cause for this, but it was noticed that the barometer was falling; the ships closed, and got into communication again. Atmospheric disturbances were then registered on both sets of instruments, and on the ships opening out again, no signals were obtained over 20 miles. The trials were

concluded shortly after, owing to intervening land. A few hours later a heavy winter gale came on, and its approach had evidently been foretold by the falling barometer, the loss of distance in signalling, and the electrical disturbances in the atmosphere, as shown by the signals received on the instruments. No lightning flashes were observed.

On another occasion, during a period of strong but intermittent atmospheric effects, no signals were obtainable between two ships up to the usual maximum signal distance. When separated 50 per cent. beyond this distance, and immediately after a particularly strong and persistent series of electrical discharges, the latter half of a signal which was being transmitted very slowly, was correctly deciphered at a distance then considered phenomenal, with the instruments employed at the time. A few minutes later, the atmospheric effects vanished, and with them all signs of further signals, till the ships had closed to their usual signalling distance. This demonstrates that the actual electrical discharges do not of themselves reduce the signalling distance or transmission of the waves at all times, but that they may, under some circumstances, assist that transmission, possibly by a cumulative effect of the waves emitted by the discharges on the waves emitted by the transmitter, these combining and increasing the effect in the receiver. I have recorded several similar results, which I cannot attribute to any other cause.

Another effect which reduces the usual signalling distance, is one that I attribute to the presence of material particles held in suspension by the water spherules in a moist atmosphere.

The Mediterranean Sea is, for days together, frequently exposed to the force of the scirocco wind; this south-easterly wind is laden with damp, and often charged with salt from spray, and dust particles from the African coast. During the continuance of these winds, the maximum signal distance is generally less than in winds (wet or dry) from any other quarter, the proportional distance being from about 60 to 80 per cent. The effect of a scirocco wind can be and is allowed for in practical wireless telegraphy.

The causes that I have considered above, and which all tend to lessen the maximum signalling distance that would obtain under more favourable circumstances, may all be attributed to influences which are beyond the control of the designers or operators of the instruments used in wireless telegraphy.

I have now, however, to consider a phenomenon which I can only assign to the apparatus in which the waves of electrical induction are generated. This phenomenon manifests itself by the gradual weakening and occasionally by the total cessation of signals, as the distance between the two ships increases, up to a certain point, and their reappearance as the distance is still further increased; in the majority of cases, the weakening of signals occurs at, or about, half the signalling

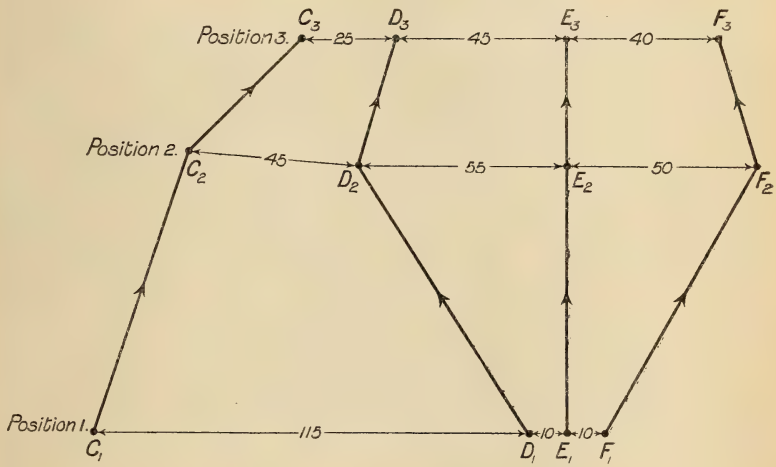
distance in the open sea, under the same circumstances, which circumstances include the direct connection of the aerial wire to one ball of the induction coil used for transmission.

The three following examples are typical cases. Units of distance are given in lieu of nautical miles.

(*a.*) A ship, A, steamed away from a station, B, to ascertain the maximum distance at which she could receive signals in the open sea.

At 48 units of distance, the signals weakened, at 57 they ceased, at 65 they appeared again, and were kept up to 100 units of distance.

(*b.*) Four ships, C, D, E, F, steered as shown in the diagram, the



maximum signalling distance between each pair being about 100 units of distance.

(The results of the signals transmitted by D are those specially to be considered.)

In position (1) D's signals were received by E F, not by C.
" " (2) " " " " " F, " E C.
" " (3) " " " " " C F, " E.

C did not commence signalling before reaching (2) and her signals were received by D and E, and maintained by them to position (3) when the trial was finished.

E's signals, which were few in number, were received by C and D in (3), but not by D in (2).

(*c.*) In the third example, ships D and F carried out a similar trial independently. Between 45 and 55 similar units of distance no signals could be exchanged either way, though at 60 units and above, and below 40, the signalling was perfect.

To further verify that it was the system of transmission that was the cause of this cessation of signals, a syntonic method, of the same approximate frequency of transmission, though of rather less power, was used alternately with the other system. Signals were exchanged perfectly with the syntonic method, but on reverting to the other method, the signals again ceased.

This was tried repeatedly with identical results. Many other similar cases have been recorded, but the effects are not always so equally well marked, even under identical circumstances.

I consider this effect is due to want of synchronism in the oscillatory discharge between the spark balls of the transmitter. This want of synchronism has also been observed by others in the photographs of oscillatory spark discharges. C. Tissot* especially remarks that, in his apparatus (presumably used for a wireless telegraph transmitter), the images of the successive sparks are not equidistant, and that the first interval is always greater than the other intervals, which also decrease very slightly. This implies that the first wave emitted is longer than the second, and so on. Owing to the rapid damping of our form of transmitter, probably only the first two or three waves emitted are of any practical value in exciting the coherer in wireless telegraphy at a distance of 30 miles; and to excite it at such distances with the power used in these transmitters, it is probably essential that the effects of the successive waves should be cumulative in their action, and for them to be so they must syntonise with the natural period of oscillation of the receiving circuit, which period, in the cases under notice, was the mean frequency of the waves emitted by the transmitter as nearly as this could be practically adjusted.

Consider the first two waves emitted, or the interval between the first and fifth sparks of the oscillatory discharge, when the third one is not spaced midway between them; the resulting waves, differing but little in length, and moving with equal velocities and in the same direction, leave a point "O" (the spark gap), the second starting a mean wave-length behind the first one, and in the same phase; at some fixed point, "P," in their path, owing to the difference in their length, the two waves will pass that point in the opposite phase, and at a point, "Q," approximately double the distance from "O" that "P" is from "O," they will pass "Q," again in the same phase, and so on, as at all points the second wave is a mean wave-length behind the first one. *To excite the coherer*, under the conditions presumed to be necessary for long distances, the impulses due to these waves must syntonise with the natural period of oscillation of the receiving circuit, and therefore these successive waves must pass by that circuit (wherever it may be), with the second following in the same phase as the first, or nearly so,

* 'Comptes Rendus,' vol. 132, p. 763 (25/3/01); and vol. 133, p. 929 (2/12/01).

otherwise the tendency of the second one will be to weaken or annul the effect of the first one.

At the point "P," therefore, when the waves are in opposite phase, it may be expected that signals will be weak, and at "Q," when they are in phase, they *may be* strong, but, owing to "Q's" distance from "O" being double that of "P," the effect of each individual impulse at "Q" is only half its effect at "P," and "Q" *may be* the maximum distance from "O," at which the cumulative effect of the successive waves will excite the coherer, even when they are in phase and in perfect syntony with the receiver circuit.

I have not yet been able to investigate the exact cause of the non-synchronous emission of the waves, but I attribute these "zones of weak signals" (as I term them) to this non-synchronous emission of the waves, and to the rapid damping of this form of transmitter, and would observe that when using my syntonic transmitter, in which the damping is less rapid, I have never noticed these effects.

A point of interest, which has also great effect on the signalling distance, is the efficiency of the earth connection of both the transmitting and receiving instruments. Fortunately for the system, on board a modern ship there is no difficulty in obtaining an almost perfect earth connection when the ship is at sea. In dry dock, however, there is, in fine weather, a great difficulty in doing so, and the effects of the bad earth with the ship in dock, on the signals, are extremely marked, both for transmitting and receiving, reducing the distance as low as to 25 per cent. of the distance with the ship afloat.

A similar effect due to drought has been observed with some shore stations, where, according to my experiences, the maximum signalling distances have always been obtained during wet seasons of the year.

A typical example is given :—

On one particular occasion, towards the end of a very dry summer (last year), the maximum signal distance between a certain ship and station, 500 feet above the sea, was 38 miles, the usual distance having previously been 68 miles. Two days later, during which time no alterations whatever had been made to the adjustments of the instruments, but which included 24 hours of heavy rain, the maximum distance obtained was 70 miles, which has since been maintained.

Repeated experiments with and without earths on the transmitter and receiver have shown me that, in the open sea, signals may be obtained up to 50 or 60 per cent. of the full distance, without earths on the receiver, though such a large proportion is unusual, the average being 30 per cent. A condenser of suitable capacity acts nearly as well as a good earth; without an earth on the transmitter, the percentage of distance has never exceeded 15 per cent. Using good earths, but no aerial wire whatever on the receiver, or near it, signals have never been obtained over 3 miles. With no aerial wire

on the transmitter I have never known a signal to be received on board another ship over 2 miles distant.

My experience demonstrates most clearly, and with no marked exception, that, for signalling any distance beyond a few miles, the combination of aerial wires and good earths is essential, for both transmitting and receiving instruments.

Summary.

The results of my observations may be briefly summed up as follows:—

(1) That intervening land of any kind reduces the practical signalling distance between two ships or stations, compared with the distance obtainable in the open sea, and that this loss in distance varies with the height, thickness, contour, and nature of the land; and that, based on the results of these observations, it may be concluded that some of the waves of electric induction, transmitted by wireless telegraphy, may pass through, over, and possibly round the land, and are comparable to the passage of ocean waves through or over a reef, or round high land, which waves proceed along their course with diminished energy, after passing such obstructions.

(2) That material particles, such as dust and salt held in suspension in a moist atmosphere, also reduce the signalling distance, probably dissipating and absorbing the waves.

(3) That electrical disturbance in the atmosphere also acts most adversely to the regular transmission of these waves, in addition to affecting the receiving instruments by lightning discharges.

(4) That a system of transmission in which the oscillations are rapidly damped is irregular in its action on distant receivers, owing to the irregularity of the train of waves giving rise to different types of disturbance at different parts of their path, which may not have at certain points the necessary cumulative effect on the receiving circuit.

(5) That the earth's function in the transmission of waves is most important; but that its importance is secondary to that of the aerial wire, or capacity insulated in the air above the surface of the surrounding sea or earth.

I have to thank many of my brother officers and men in H.M.'s Navy for the valuable assistance they have ungrudgingly given me in carrying out my experiments.

"On Electromotive Wave accompanying Mechanical Disturbance in Metals in Contact with Electrolyte."* By JAGADIS CHUNDER BOSE, M.A., D.Sc., Prof. Presidency College, Calcutta. Communicated by Professor REINOLD, F.R.S. Received March 14,—Read May 15,—Revised May 21, 1902.

Take a rod of metal, and connect the two points A and B with a galvanometer by means of non-polarisable electrodes. (Fig 1, *a*.) If the point O is struck, a wave of molecular disturbance will reach A and B. It will be shown that this is attended by a wave of electric disturbance. The mechanical and the attendant electrical disturbance will reach a maximum and then gradually subside. The resultant effect on the galvanometer will be due to $E_A - E_B$ where E_A and E_B are the electric variations produced at A and B. The electric changes at

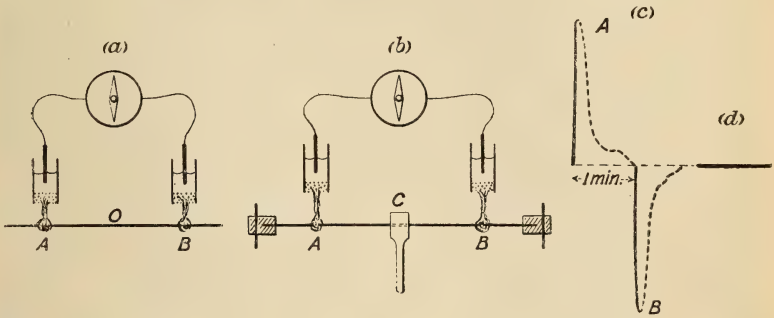


FIG. 1.—In (*a*) mechanical disturbance applied at O produces similar electrical disturbances at A and B; there is no resultant effect. In (*b*) owing to a clamp, disturbance applied at A cannot reach B. A tap or vibration imparted to the end A produces responsive current which flows in the wire from the unexcited B to the excited end A. Disturbance of B gives rise to a current in the opposite direction. (*c*) gives the record of the response to equal stimuli applied to A and B. The ascending part of the curve shows the effect of stimulus, the falling part shows recovery. (*d*) Simultaneous stimulation of A and B gives no resultant response. (In the records dotted lines represent recovery.)

A and B will continuously balance each other, and the resultant effect on the galvanometer will be zero, (1) if the mechanical disturbance reaches A and B at the same time and with the same intensity, (2) when the molecular condition is similar at the two points, and (3) when the rate of rise and subsidence of disturbance is the same at the two

* A preliminary account of this investigation was given in a paper "On Response in Inorganic Substances," read before the Society, June 6, 1901.

points. In order that a resultant effect may be exhibited in the galvanometer, matters have to be so arranged that (1) the disturbance may reach one point, say A and not B, and *vice versâ*. This may be accomplished by the method of block. Again, a resultant differential action may be obtained even when the disturbance reaches both A and B, if the electrical excitability of one point is relatively exalted or depressed by physical or chemical means: we thus have two other means of obtaining a resultant effect, (2) by the method of relative exaltation, (3) by the method of relative depression.

Method of Block.

The electromotive effect described below may be obtained with all metals. A piece of "tin" wire (an alloy of tin and lead used as electric fuse) will be found to give very good results. A specimen of wire 1 mm. in diameter, 10 cm. in length, is mounted in the apparatus. (Fig. 1, *b*.) Two strips of cloth moistened with water or dilute salt solution are securely tied round two points A and B. They hang loose from EE′ (non-polarising electrodes—Zn in $ZnSO_4$ solution), so that there is no pull on the wire. Special precautions are taken so that there is no variation of contact. If a sharp tap be given to the side A, a transitory electrical current in response to the disturbance will flow round the circuit, which under normal conditions will be found to flow *in the wire* towards the more excited end A. Disturbance of B will give rise to a reverse current. For quantitative measurement it is necessary to have the intensity of stimulus maintained uniform or increased or decreased in a definite manner. Instead of a tap, the stimulus of torsional vibration is more satisfactory. By maintaining the amplitude of vibration constant or increasing or decreasing the amplitude, we may either keep the stimulus constant or increase or decrease it in a definite manner. I shall first describe some of the typical results which may be obtained with the simple "straight wire form" of the apparatus. If worked with care it will give consistent and satisfactory results. For quantitative measurements requiring the greatest exactitude the "cell form," to be presently described, will be found preferable.

Recording Apparatus.—The galvanometer used is a sensitive dead-beat D'Arsonval; the period of complete vibration of suspended coil under experimental conditions is 11 seconds. The records are taken by means of a cylindrical modification of the response recorder described in my previous paper,* or by means of photography. In the latter method, a clockwork moves the photographic plate at a uniform rate and a curve is traced on the plate by the moving galvanometer spot

* "On the Continuity of Effect of Light and Electric Radiation on Matter,'* Roy. Soc. Proc.,' vol. 70, p. 159.

of light. The disturbance of molecular equilibrium caused by the stimulus is attended by an electromotive variation, which gradually disappears on the restoration of the molecules to equilibrium. The rising portion of the response curve shows the electromotive effect, due to stimulus, and the falling portion the recovery. The ordinate represents the electromotive variation, and the abscissa the time.

Experiments to Exhibit the Balancing Effect.

If the wire has been carefully annealed, the molecular conditions of different portions are approximately the same. Every portion of the surface will be found nearly iso-electric. If the wire be held near the middle by the clamp, and a vibration through an amplitude of, say, 90° be given to the end A, an upward deflection will be produced; if a vibration of 90° be given to B, there will be produced an equal downward deflection. (Fig. 1, *c.*) If both the ends are simultaneously vibrated, the electromotive variation at the two ends will continuously balance each other, and the galvanometer spot will remain quiescent. (Fig. 1, *d.*) The clamp may even be removed, and the wire vibrated as a whole; the stimulation of A and B being the same, there will be no resultant deflection. Having found the balancing point for the clamp (which is at or near the middle), if the clamp be now shifted to the left, on simultaneous vibration of A and B, the A effect will be relatively stronger (inasmuch as the angular vibration of A is increased and that of B decreased), and there will be produced a resultant upward deflection. Thus keeping the rest of the circuit untouched, by merely moving the clamp from the left, past the balancing position to the right we get either a positive or zero or a negative resultant effect. This can be repeated any number of times. The experiment shows further that when the amplitude of vibration is kept constant, the intensity of electromotive effect is increased by shortening the wire. A thick wire produces a stronger response. The direction of the current of response in the wire is in the majority of metals under normal condition, from the relatively less to the relatively more excited point.

The form of the response curve, stimulus remaining constant, is modified by the molecular condition of the wire. A wire in a sluggish condition shows feeble response, the recovery is also slow. The same wire after it has been vibrated for a time exhibits stronger response. The period of recovery may also then be hastened. Longer time is required for recovery from the effects of a stronger stimulus.

Comparison of Electric Excitability of Two Points by the Method of Balance.

As has already been said, when the clamp is put at the balancing

position, alternate equal stimulations of A and B produce equal and opposite electromotive effects, and when the two ends are stimulated simultaneously there is no resultant effect.

Increased Excitability produced by Preliminary Vibration.—If now one-half of the wire, say the A half, be vibrated for a time, the electric excitability of that half will be found to be more or less permanently augmented, presumably by increased molecular mobility conferred by vibration. The response of A would now be found to be very much enhanced, as compared with its previous response, the response of B remaining the same as before. If now both the ends are simultaneously vibrated, the balance which previously existed will be found to be disturbed, the resultant showing that A has been rendered the more excitable.

If B be now vibrated for a time, the former approximate balance will be re-established by the enhanced responsiveness of B. Thus in the following experiment with the clamp at the approximate balancing point—

	Response of A.	Response of B.	Resultant response.
Approximate balance	+ 5 divisions	−4·5 divisions	+0·5 divisions
After the end A has been vibrated	+10·5 „	−4·5 „	+6·0 „
After the end B has been vibrated for an equal length of time	+10·5 „	−9·5 „	+1·0 „

Effect of Chemical Reagents.—It will be shown that keeping the electrolyte by which contact is made constant, the electric excitability of the wire depends on the molecular condition of the wire. Certain electrolytes, such as dilute solution of NaCl, dilute solution of bichromate of potash, &c., are normal in their action, that is to say, with such contacts the response to stimulation is practically the same as with distilled water contact.

Contact made with dilute NaCl solution may therefore be regarded as the normal contact. There are again certain chemical reagents which enhance the electrical excitability; others on the contrary produce great depression, or abolition of excitability.

Electric Comparator.

We may compare the relative electric excitability conferred by chemical reagents by the method of balance. Having previously obtained a balance (with water or dilute NaCl solution contacts

at A and B), one contact, say A, is touched with a few drops of Na_2CO_3, which is an exciting agent. The electric excitability of A will now be found to be greater than that of B; on simultaneous vibration of A and B there will be a disturbance of balance, giving rise to an upward deflection (current of response towards the more excitable A).

	Response of A.	Response of B.	Resultant response.
Both contacts of normal saline	+12	−12	0
Contact A touched with Na_2CO_3 solution.....	+32	−12	+20

Similarly, when A is depressed by a trace of oxalic acid the electric excitability of A is less than that of B, the resultant deflection being now downwards (current of response towards the relatively more responsive B). It is to be remembered that in all cases *the resultant current of response in the wire is towards the more excitable point.*

An interesting line of investigation rendered possible by a modification of method of balance described above is to compare the relative excitability induced by various chemical reagents, the influence of the same reagent of different strengths, and the modification of the effect caused by the duration of application. We may thus compare the effect of the reagent in relation to the normal effect of water or dilute NaCl solution. There is again an extremely delicate method of comparison of the relative effects of a series of compounds like Na_2CO_3, K_2CO_3, &c. Balance having been previously obtained between the normal sensitiveness of A and B, the two different solutions are now applied at the two points; the slightest difference in their relative action is at once exhibited by the upsetting of the balance during stimulation, the direction of the resultant deflection indicating the more exciting reagent.

Resultant Response by Method of Relative Depression or Exaltation.

From what has been said, it will be seen that by rendering A and B unequally excitable, a resultant response may be obtained. The block may be abolished, and the wire may be vibrated as a whole; the response will now be due to the differential effect at A and B. To produce difference in excitability we may subject one point, say A, to a preliminary vibration, or apply at the point a suitable chemical reagent. By the application of the latter there will be a small P.D. between A and B: this will simply produce a displacement of the zero. (By means of

a potentiometer the galvanometer spot may be brought back to the original position.) The shifting of the zero will not affect the general result. The direction of this more or less permanent current, due to the small P.D., gives no indication of the direction of current of response; the direction of the latter is determined by the rule that the responsive current flows towards the more excitable point. The effect of the mechanical stimulus is to produce a transient electromotive force which is algebraically superposed on the existing P.D. The deflection will take place from the modified zero, to which the spot returns during recovery. I give four records (fig. 2): in (*a*) A is touched with Na_2CO_3 (which is an excitant): a permanent current flows from B to A: response to stimulus is in the same direction as the permanent current (positive variation); in (*b*) A is touched with a trace of oxalic acid (which depresses the excitabilily), the permanent current is in the same direction as before, but the current of response is in the opposite direction (negative variation); in (*c*) A is touched with dilute KHO (3 parts in 1000), the response is exhibited by a positive variation; in (*d*) A is touched with stronger KHO (3 parts in 100), the response is now exhibited by a negative variation. The last two apparently anomalous results are due to the fact (which will be demonstrated later) that KHO in minute quantities is an excitant, while in larger quantities it is a depressant.

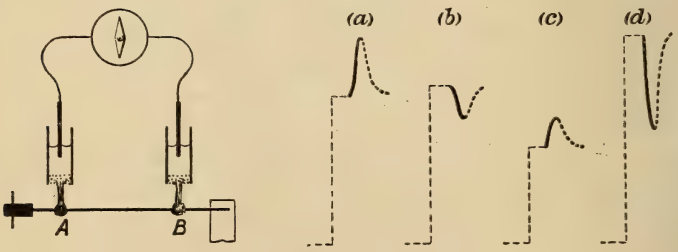


FIG. 2.

(*a*) Response when A is treated with sodium carbonate—an apparent positive variation

(*b*) „ „ „ „ „ oxalic acid—an apparent negative variation

(*c*) „ „ „ „ „ very dilute potash—positive variation.

(*d*) „ „ „ „ „ strong potash—negative variation.

This response is up when A is more excitable and down when B is more excitable.

Lines thus ——— indicate direction of permanent current.

	Permanent current.	Current of response.
A treated with sodium carbonate	←	←
,, ,, ,, oxalic acid	←	→
,, ,, ,, very dilute potash	←	←
,, ,, ,, strong potash	←	→

Current of response is always towards the more excitable point.

Detection of Traces of Physico-chemical Change.

I will now describe an experiment which will show in a striking manner how exceedingly delicate is the method of electric response to stimulation, and how by its means we can detect and measure traces of physico-chemical changes in different parts of the same solid. Take a wire and touch two points, one with Na_2CO_3 solution the other with oxalic acid. Wash the wire. There is no trace left of the previous treatment. Let one contact be permanently made at a normal or previously unacted point N. Let the other exploring contact be moved

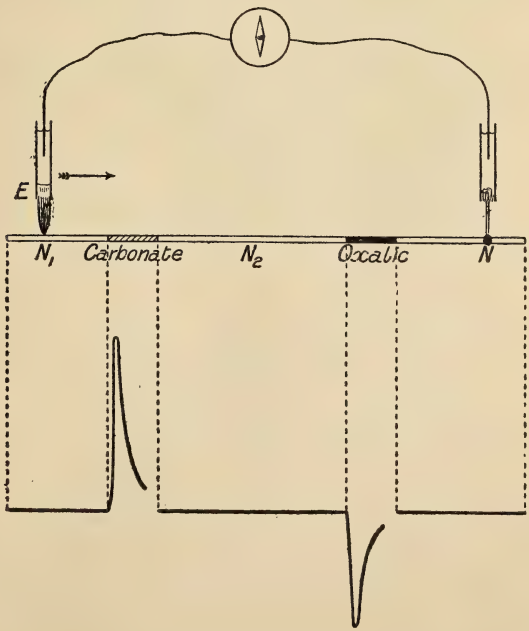


FIG. 3.—Electro-molecular Explorer.

along from the other end towards N, the wire being mechanically stimulated during the test. The galvanometer spot remains quiescent as long as the exploring contact is over normal areas. But as soon as it touches the zone on which is impressed the invisible image of physico-chemical change, the differential effect of stimulus at once reveals it by producing a vigorous movement of the galvanometer spot. At N_1 there was no movement, but there was an upward movement of response when the explorer came over "Carbonate." As the explorer passed on to N_2 there was a cessation of movement, but when it reached the area marked "Oxalic" there was a vigorous movement downwards (fig. 3).

Interference Effects.

I have already described a case of interference in the galvanometric effect when the two points A and B in similar molecular conditions are simultaneously acted on by the same mechanical stimulus. Under these conditions the electric variation at the two points *continuously* balance each other, and there is no resultant effect.

When one point is acted on by a chemical reagent, not only is its electric excitability changed, but its time relations—its latent period, the time-rate of its acquiring the maximum electric variation, and the recovery from the effect of stimulus—will also be modified. Using the block method, we may place a drop of excitant Na_2CO_3 on A and depressant KBr on B. On simultaneous vibration of A and B, the A effect being relatively much stronger than B effect, the resultant would be an upward deflection. But on shifting the balancing clamp away from A (thus decreasing the stimulation intensity of A and increasing that of B) we may find a point where the A effect is equal and opposite to the B effect. But owing to change of time relations, simultaneous vibration of A and B will no longer give a continuous balance; instead we obtain a diphasic variation. The diphasic curve thus obtained is exactly the same as the resultant curve deduced from the algebraic summation of the A and B curves obtained separately.

Continuous Transformation from Positive to Negative through an Intermediate Diphasic Response.—In the following record, fig. 4, I succeeded in obtaining a continuous transformation from positive to negative phase by continuous change in the relative sensitiveness of the two contacts. I found that traces of after effect due to application of Na_2CO_3, even after it is washed off, remain for a time, the increased sensitiveness conferred disappearing gradually. Again, if we apply Na_2CO_3 solution to a fresh point, the sensitiveness *gradually* increases. There is another interesting point, viz., that the beginning of response is earlier when the application of Na_2CO_3 is fresh. In the experiment whose record is given, the wire is held at one end, and successive uniform vibrations imparted to the wire as a whole at inter-

vals of one minute by means of a torsion head at the other end. (See fig. 2.) Owing to after effects of previous applications of Na_2CO_3, the sensitiveness of A is at the beginning great, hence the resultant response is at the beginning positive or upward. Dilute solution of Na_2CO_3 is next applied to B. The response of B (down) begins earlier, and con-

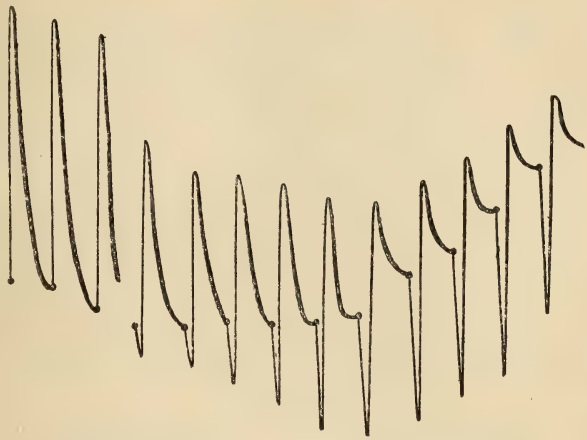


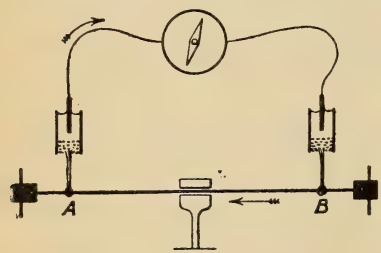
FIG. 4.—Transformation from positive to negative through intermediate diphasic variation. Thick dots represent times of application of stimulus.

tinues to grow stronger and stronger. Hence, after this application, the response shows a preliminary negative twitch of B followed by positive variation of A. The negative grows continuously. At the fifth response, the two phases, negative and positive, of the double response become equal; after that, the negative becomes very prominent, the positive dwindling into a feeble after vibration.

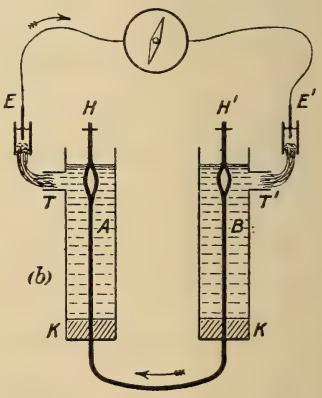
Modification of the Apparatus into "Cell Form."

The series in fig. 5 explains the transformation from the "straight wire" to "cell" form. The wires A and B, cut from the same piece, are clamped separately below; vibration of A (the amplitude of which is measured by a graduated circle) gives rise to a responsive current in one direction, vibration of B gives rise to a current in an opposite direction. Every experiment may thus be verified by corroborative and reversal effects. The electromotive effect varies with the substance, and is sometimes considerable, for example, with "tin," a single vibration may give rise to as high a value as 0·4 volt or more. The intensity of response does not depend on the chemical activity of the substance, for the electromotive variation in the relatively inactive tin is greater than in zinc. Again, the sign of response, positive or

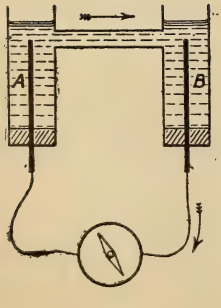
negative, is sometimes modified by the molecular condition of the wire (see below). In the modified form of apparatus the wires in the cell are immersed to a definite depth in the electrolyte; there is thus a perfect and invariable contact between the wire and the electrolyte. The wire in the cell is clamped below, and torsional vibration gives rise to a strong electrical response. If the wire be now carefully unclamped, and the vibration repeated as before, there will now be found no electrical effect. As all the rest of the circuit was kept absolutely the same in the two different sets of experiments, these results conclusively prove that the responsive electromotive variation is solely due to the mechanical stimulation of the acted wire. The excitatory effect due to the disturbance persists for a time. This may be shown by keeping the galvanometer circuit open during the application of vibration, and completing it at various short intervals after the cessation, when a persisting electrical effect diminishing rapidly with time will be observed. When the wire is brought to the normal condition, successive responses to uniform stimuli are, in the case of metals which, like tin, show no fatigue, exactly the same. I usually interpose a high external resistance, varying from 1 to 5 megohms, so that the galvanometer deflections may be proportional to the electromotive variations;



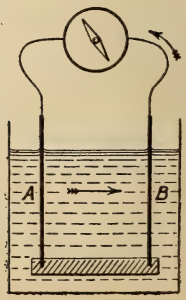
(a)



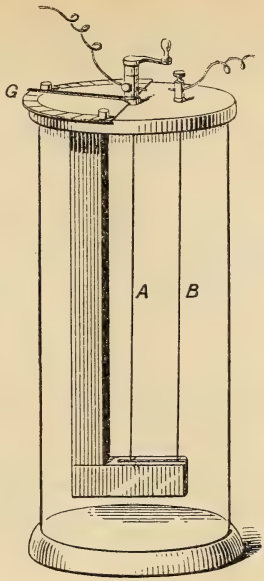
(b)



(c)



(d)



(*e*)

FIG. 5.—Successive modifications of the "straight wire" ending in "cell form." (*b*) shows how the ends of A and B of the wire may be vibrated by ebonite clip-holders, H and H′. When A is excited, current of response *in the wire*, normally speaking, is from the unexcited B to the excited A. The stimulated wire becomes zincoid. Note that though the current of response is constant in direction, the galvanometer deflection in (*d*) will be opposite in direction to (*b*). In (*e*) is shown one of the two graduated circles by which the amplitude of vibration is measured.

the internal resistance of the cell and the variation of that resistance by the addition of chemical reagents being thereby rendered quite negligible. Ordinarily I use tap-water as the electrolyte. The responses obtained with tap-water are practically the same as those obtained with distilled water. Zinc wires in $ZnSO_4$ solution give responses similar in character to those given by, for example, Pt or Sn in water.

Character and Intensity of Response dependent on Molecular Condition.

The following experiments show how intimately the response phenomena is connected with the molecular condition of the acted wire:—

Effect of Annealing.—The following photographic record, fig. 6, shows the equal and opposite responses in A and B wires to a succession of uniform stimuli. Hot water was now substituted for the cold water (too high a temperature temporarily reduces the response); the cell

was then allowed to cool to its old temperature, when a second series of responses were taken, the stimulus intensity being the same as before. It will be seen how the responses are enhanced by annealing.

FIG. 6.—Series of responses, to uniform stimuli, of both A and B wires, before and after annealing.

Effect of Previous Vibration.—The increased sensitiveness conferred by previous vibration has already been referred to before. I give below a record (fig. 7) obtained with platinum (I have obtained similar

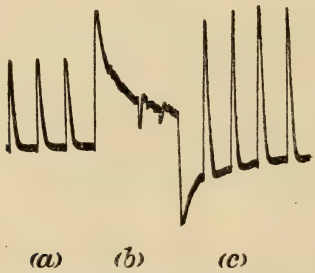


FIG. 7.—Photographic record showing the effect of continuous stimulation in enhancing response (Pt). Each curve shows response (followed by recovery), the stimulus being kept constant throughout. The series of responses (*a*), enhanced to series (*c*) after continuous vibration (*b*).

records with other metals), which clearly shows how the response is enhanced after preliminary vibration.

Sometimes the wire gets into a very sluggish condition, when the response almost disappears; in other words, owing to some molecular modification, responsiveness is reduced from the normal positive value (by positive is meant that the acted wire becomes zinc-like or is zincoid) to zero. In these cases annealing or preliminary vibration are usually effective in transforming the sensitiveness from zero to a positive value.

Abnormal Response.

But the modification of which I have spoken does not stop short of mere abolition of responsive power, but sometimes proceeds further, and actually reverses the sign of response—the excited wire becoming cuproid. This abnormal effect is sometimes found even in fresh unannealed wires, when feeble vibrations (of amplitude of 5° or less) may give rise to a negative response, but vibration of stronger intensity gives rise to the usual positive response. At other times the molecular modification is more pronounced, and there is a persistent reversal of response.

But even in such cases long-continued vibration transforms the abnormal negative to the normal positive. I give below photographic records which exhibit this. In fig. 8, α, the transformation took place during continued vibration. To detect the point of transformation, I experimented with a platinum cell which exhibited the abnormal effect, and took a long series of records of responses to uniform vibrations acting at intervals of a minute. In the record (fig. 8, β) I have been able to catch the point or rather points of transition.

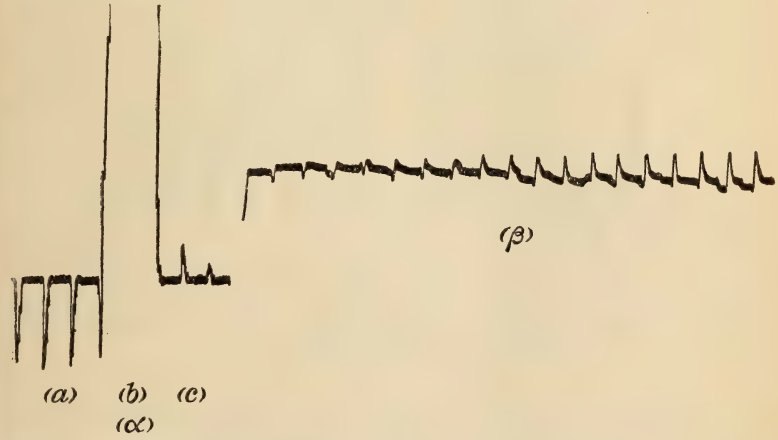


FIG. 8.—(α) Abnormal negative (downward) response (a) converted into normal positive (upward) response (c) after continued vibration (b) (tin).
(β) Shows points of transition from the abnormal negative to the normal positive (platinum).

Thus we may distinguish the following typical cases. Beginning with the case of extreme molecular modification, we have (1) a condition which gives rise to negative response; after continued vibration the negative becomes less negative, and ultimately becomes converted into positive: (2) an irresponsive or neutral condition; vibration or annealing transforms it into positive: (3) a sluggish, feebly positive, becoming more and more positive after continued vibration: (4) a

steady and permanent condition, when the responses are uniform: and lastly (5) when vibration is continued for too long a time, the positive tends to become less positive, the responses decline—a state of things which we designate as fatigue.

Increased Electromotive Variation with increased Intensity of Stimulus.

When the intensity of stimulus is increased by increasing the amplitude of vibration, the electric response is enhanced. The following is a pair of records (fig. 9) for increasing amplitudes from 5° to 40°, and decreasing amplitudes from 40° to 5°. The vibrations are imparted at intervals of 1 minute. It will be noticed how the response is enhanced with increasing stimulus.

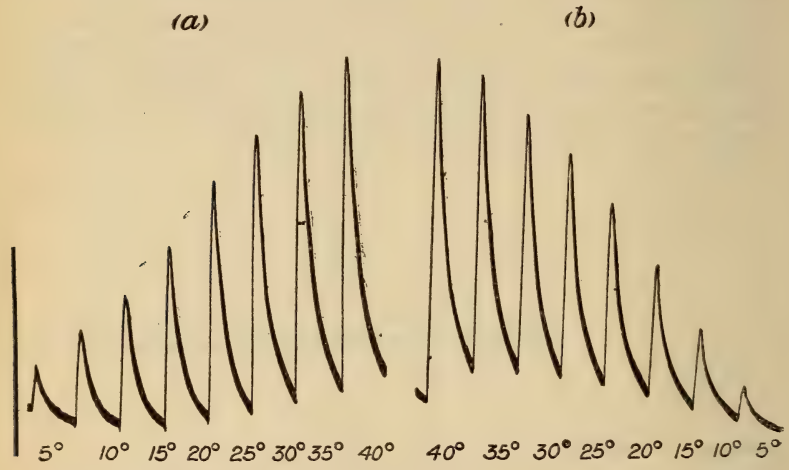


FIG. 9.—Photographic records of response curves. (*a*) from 5° to 40°, (*b*) from 40° to 5°. The vertical line = 0·1 volt.

Table I.—Showing the Increasing Electromotive Effect due to Increasing Amplitude of Vibration.

Vibration, amplitude.	Deflection. 23 dns. = 0·1 volt.	
	Ascending.	Descending.
5°	5·5	5
10	13	12
20	25·5	26
25	33	32
30	39	39
35	43	43
40	47	48

It will also be noticed that whereas recovery is complete in 1 minute when the vibration amplitude is small, it is not quite complete within that time when the vibration amplitude is large. Greater strain prolongs the period of recovery. Owing to want of complete recovery, the base line is tilted slightly upwards. This slight displacement does not materially affect the results, provided the shifting is slight. From other records taken through a greater range of stimulation, it appears that in a curve obtained with electromotive variations as ordinates and amplitudes of vibrations as abscissæ, the first part of the curve is, generally speaking, slightly convex to the abscissa (the convexity is pronounced when feeble stimulation gives negative response), it is then straight in the middle and concave in the last part. A limiting deflection is approached with high amplitude of vibration. The shape of the curve is modified by the molecular condition of the wire.

Maximum Effect.

If instead of a single vibration of a given amplitude we superpose a rapidly succeeding series, the individual effects are added up and a maximum deflection is produced which remains practically constant as long as the vibration is maintained. (A single ineffective stimulus may thus become effective by the additive effect of several.) Too long-continued vibration may cause fatigue, but during half a minute or so, the maximum effect is very definite (in tin). For a definite amplitude of vibration there is a definite maximum, which increases with the amplitude. For example, a single vibration of 5° gave a deflection of 3·5 divisions, the same when continued at the rate of four times per second gave a maximum deflection of eighteen divisions.

Again, with the same wire, a single vibration of 10° gave a deflection of 4·5 divisions, but continued vibration gave the definite maximum of 37·5 divisions. I give below a curve (fig. 10) which shows the maximum effect for different amplitudes of vibration.

Hysteresis.—Allusion has already been made as to the increased sensitiveness conferred by preliminary vibration. Being desirous of finding out in what manner this is brought about, I took a series of observations for an entire cycle, that is to say, a series of observations were taken for maximum effects, starting from 10° and ending in 100°, and backward from 100° to 10°. Effect of hysteresis is very clearly seen (fig. 10, A); there is a considerable divergence between the forward and return curves, the return curve being higher. On repeating the cycle several times, the divergence is found to be very much reduced, the wire on the whole is found to assume a more constant sensitiveness. In this steady condition, generally speaking, the sensitiveness for smaller amplitude of vibration is found to be greater than at the very beginning, but the reverse is the case for stronger intensity of vibration.

Effect of Annealing.—I repeated the experiment with the same wire, after pouring hot water and allowing it to cool to the old temperature. It will be seen from the cyclic curve (fig. 10, B), (1) that the sensitiveness has become very much enhanced; (2) that there is relatively less divergence between the forward and return curves. Even this divergence practically disappeared at the third cycle, when the forward and backward curves coincided (fig. 10, C). The above results show in what

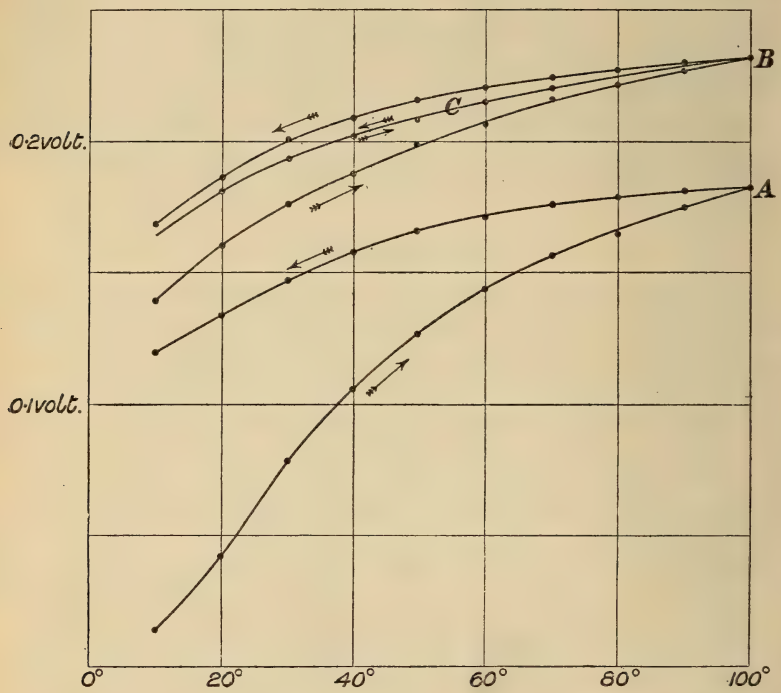


FIG. 10.—Cyclic curves for maximum effects due to increasing and decreasing amplitude of vibration. A, fresh wire; B, after annealing; C, the same after three cycles. Abscissa represents the amplitude of vibration; the ordinate represents the corresponding electromotive variation.

manner the excitability of the wire is enhanced by purely physical means.

It is very curious to notice that the substitution of Na_2CO_3 solution as electrolyte produces results very similar to that produced by annealing; that is to say, not only is there a great enhancement of sensitiveness, but there is also a reduction of hysteresis. Another curious point is that, whereas with ordinary fresh wire the addition of Na_2CO_3 greatly enhances the sensitiveness, after the wire has been annealed there is comparatively little further increase of sensitiveness due to the addition of the reagent.

Effect of Chemical Reagents.

I shall now give photographic records of a few typical cases which will graphically illustrate the influence of chemical reagents. The mode of procedure is as follows. The cell is filled with water, and photographic records are taken of responses to single vibrations of constant amplitude, applied to one of the two wires at intervals of 1 minute. The responses will be found uniform. Chemical reagent is now added, and similar vibrations applied as before. The responses will exhibit either an increase or diminution, depending on the exciting or depressing power of the reagent. It is also quite easy to obtain duplicated results by alternately vibrating the A and B wires. Uniform responses, alternately positive or negative, will be first obtained; after the addition of reagent *both* will exhibit either an increase or a diminution. As has been said before a very high external resistance, varying from 1 to 5 megohms, is interposed in the external circuit, the slight variation of internal resistance of the cell due to the addition of the reagent being then quite negligible compared with the total resistance of the circuit. That there is no appreciable variation in the total resistance can be independently verified by applying a known electromotive force before and after the addition of the reagent, when the resulting deflection will be found to be the same in the two cases. The responsive deflections are thus simply proportional to the electromotive variations produced.

Chemical Excitants.—The following record (fig. 11) exhibits the

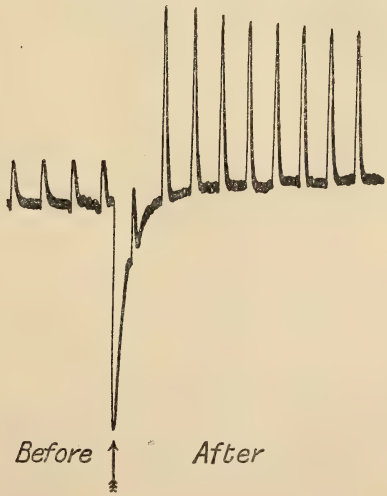


FIG. 11.—Enhanced response by the action of Na_2CO_3 solution on platinum. The intensity of stimulus is kept constant throughout. The curves to the left show the responses before, and those to the right after, the application of Na_2CO_3.

increased response due to the action of Na_2CO_3 on Pt. Another record shows an exactly similar effect on tin. The record of effect was taken 2 minutes after the application. Other records taken immediately after, show that the enhanced responsiveness takes place gradually with time. (See also fig. 7, and compare the general similarity between the enhancement of response produced by preliminary vibration and by the action of Na_2CO_3.)

Depressants.—Other reagents, like KBr (10 per cent.), produce a depression in the response. There are again others which abolish the response almost completely, for example, 3 per cent. KHO solution (fig. 12, C). One of the most effective reagents which abolishes the response is oxalic acid. The depressing effect of this reagent is so great that a strength of 1 part in 10,000 is often sufficient to produce an abolition of response.*

Opposite Effects of Varying Strengths of Solution.—The most curious effect is that exhibited by some reagents when the strength of solution is varied. This is clearly seen in the following record (fig. 12), in which

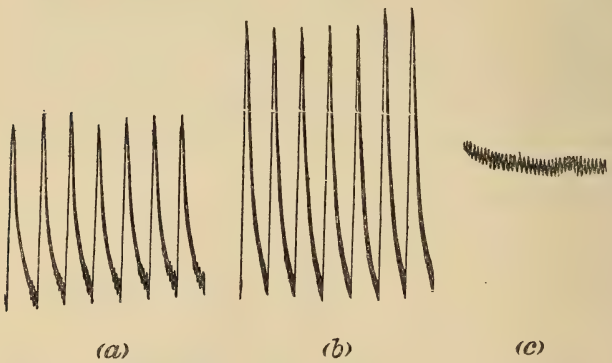


FIG. 12.—Records showing the opposite effects of weak and strong solutions. (*a*) Normal response; (*b*) increased response due to addition of 0·3 per cent. KHO; (*c*) abolition of response by 3 per cent. KHO. The trace (*c*) should have been engraved merely as a thick trace.

(*a*) gives the normal response in water. KHO solution was now added so as to make the strength 3 parts in a thousand and (*b*) shows the

* The various phenomena connected with the response in inorganic substances—the negative variation—the relation between stimulus and response—the increased response after continuous stimulation—the abnormal response converted into normal after long-continued stimulation—the diphasic variation—the increase of response by stimulants, decrease by depressors, and abolition by "poisons," so-called—all these are curiously like the various response phenomena in living tissues. A complete account of the mutual relation between the two classes of phenomena will be found in a work to be shortly published, "On the Response in the Living and Non-living" (Messrs. Longmans).

enhancement of response thereby produced. A further quantity of KHO was now added so as to increase the strength to 3 parts in a hundred. This caused (*c*) a complete abolition of response. (Refer to fig. 2, *c*, *d*.)

I shall now briefly mention some of the interesting points in connection with the action of chemical reagents. (1.) The effect of reagent is not only to increase or diminish the height of response, but also to modify the time relations. By the action of some the quickness with which the maximum effect is reached is enhanced, others produce a prolongation of the period of recovery. Curious effects of this in producing diphasic variation have already been mentioned. (2.) In a sensitive annealed wire the further enhancement of response by Na_2CO_3 is not so great as in a fresh wire. The effect produced by a reagent is thus seen to depend to some extent on the previous condition of the wire. (3.) A certain time is required for the full development of this effect. With some the maximum effect takes place almost instantaneously, while with others it takes place gradually. Again, the effect may with some reach a maximum, after which there is a slight decline. The after-effect of some reagents is transitory while that of others is very persistent.

It is difficult to say how much of this modification of responsiveness by various reagents is due to "physical" and how much to "chemical" cause. It has been shown that the responsive power does not depend on the chemical activity of the substance. Tin is more responsive than zinc. Pt in distilled water shows response. Strong acids and alkalies abolish response, but very dilute KHO enhances response. Oxalic acid, even in minute quantities, abolishes it. Neutral Na_2CO_3 enhances it, but dilute NaCl produces no change in the normal response. The responses of the same wire under different physical modifications are different, and under certain molecular modification the sign of response is even reversed. But continued vibration makes the response normal. Again, the enhancement of response produced by Na_2CO_3 can be closely imitated by the effect of continued vibration. The stimulating effect of this reagent gradually attains a maximum. The after-effects of some chemical reagents persist even after all traces have been removed. For example, in a certain experiment the A and B wires gave each a response of 23·5 divisions. The wires were lifted from the cell, and the A wire touched with dilute oxalic acid. It was then rubbed under tap-water with a piece of cloth, so as to remove all traces of the acid. On replacing the wire on the cell, the responsiveness of the untouched B was found unchanged, but that of A had undergone an abolition. The depressing action is often so persistent and deep that I have on many occasions failed to revive the response even after the surface layers had been removed by rubbing the wire

with emery-paper. The wire so treated may, after a long time, exhibit partial recovery of its responsive power.

The facts described above seem to show that the enhancement or depression of response may, at least to a considerable extent, be due to the increase or diminution of molecular mobility conferred by the chemical reagents. With a given stimulus, the height of response and the form of the response curve will be determined by the element of molecular friction. In connection with this, it is instructive to obtain records of the vibrations of a torsional pendulum, the friction of which may be gradually increased by immersing the pendulum more or less in a viscous fluid or sand. The various types of response-curves in metals are found to be very similar to those thus obtained.

Of these I give an interesting example. With moderate friction the successive curves obtained with the pendulum are like those given in the left of fig. 13 (*a*). With increased friction the height of the curve is diminished, the maximum is reached later, and the recovery is prolonged (like the curve in the right of fig. 13 (*a*)). With still greater friction the recovery is arrested.

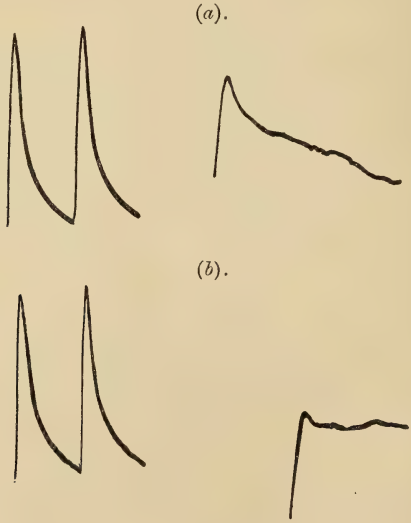


Fig. 13.—Photographic records showing the effect of "molecular arrest." The two curves to the left of each set show the normal response; curve to the right in (*a*) shows partial and in (*b*) complete arrest, produced by the reagent.

It would appear as if the reagents which abolish response in metals produce a similar molecular arrest. The following photographic records seem to lend support to this view. If the oxalic acid be applied in large quantities, the abolition of response is complete; but

on carefully applying just the proper amount, I find that the stimulus evokes a responsive electric variation which is less than the normal, and the period of recovery is very much prolonged from the normal 1 minute before to 5 minutes after the application of the reagent (fig. 13 (*a*)). In the next record (fig. 13 (*b*)) the arrest is more pronounced, *i.e.*, there is now no recovery. Note also that the maximum is attained much later. Stimuli applied after the arrest produce no effect, as if the molecular mechanism became locked up.

Résumé.

1. Molecular disturbance produced by mechanical stimulus gives rise to an electrical disturbance. In the majority of cases, under normal conditions, the responsive electrical current in a wire is from the less to the more disturbed.

2. Response may be obtained by (1) method of block, (2) by methods of negative or positive variation.

3. The electromotive variation disappears on the cessation of disturbance.

4. The intensity of the electrical variation produced by a given disturbance is modified by the molecular condition of the wire. Annealing, or previous continued vibration, enhances the electric effect.

5. The abnormal response due to molecular modification is transformed into normal by continued vibration.

6. The intensity of electromotive variation is increased with increasing intensity of stimulation.

7. In a curve—the ordinates representing the electrical effects, and the abscissæ the amplitudes of vibration—the first part is slightly convex to the abscissa, the second is approximately straight, and the third concave. With increasing stimulation there is a tendency for the electrical variation to reach a limit.

8. A maximum electrical effect is produced by continuous vibration, which is definite for a given amplitude of vibration. A curve showing the relation between the maximum effect and the amplitude of vibration exhibits the same characteristics as in the last case.

9. Hysteresis is exhibited in cyclic curves. The forward and return curves tend to coincide after several cycles. Previous annealing reduces hysteresis, and after one or two cycles the wire assumes a constant condition of sensibility.

10. Chemical reagents may profoundly modify the electric excitability. Some increase the excitability. Others depress or even abolish the excitability. The after-effects are sometimes very persistent.

11. The effect of weak solution is sometimes the opposite to that of strong solution.

12. By touching different points of the wire with different reagents, the excitability of these portions are rendered unequal. Hence a resultant electromotive variation may be obtained by vibrating the wire as a whole. The current in the wire is from the less to the more excitable.

13. By this method, invisible traces of physico-chemical change in a wire may be detected.

14. Chemical reagents not only change the excitability but the quickness of response. Two points having two different rates of excitation will thus, under proper conditions, give rise to diphasic effects.

I take this opportunity to thank the Managers of the Royal Institution for the facilities offered me to carry on the investigation at the Davy-Faraday Laboratory.

"On the Effect of a Longitudinal Magnetic Field on the Internal Viscosity of Wires of Nickel and Iron, as shown by Change of the Rate of Subsidence of Torsional Oscillations." By Professor ANDREW GRAY, F.R.S., and ALEXANDER WOOD, B.Sc., Houldsworth Research Student in the University of Glasgow. Received May 1,—Read May 15, 1902.

We can obtain information as to the nature of the magnetisation of magnetisable bodies only by testing the various hypotheses with reference to effects which it seems likely should, under these hypotheses, be produced on the physical properties of the substance. Thus, for example, the internal friction of the different parts of a solid must depend upon the size and mode of arrangement of these parts, and any alteration in their dimensions or relative arrangement ought in general to produce some change in the amount of the internal friction. Magnetisation of iron and other substances has with great probability been supposed to consist in a rearrangement and general alignment of the particles of the substance, already themselves elementary magnets, but so arranged in the unmagnetised metal as to be unproductive of any external magnetic field. It is not unusual to suppose that this unmagnetised state is one of what we may call complete absence of arrangement, and it is sometimes so represented in text-books on the subject of magnetism, where pictures are given of a perfectly confused distribution of elementary magnets, so completely mixed up as to have no preponderating magnetic moment in any one direction. Any such distribution, it is clear at once from the most elementary considerations, is impossible, as a large majority of the elementary magnets would otherwise have to remain in stable equilibrium in

unstable conditions of magnetic forces, a state of things which could only be brought about with the assistance of forces independent of magnetic action—forces which there is otherwise good reason to believe do not exist to anything like the degree which would be necessary. There are other reasons for believing that in the unmagnetised metal the elementary magnets have a perfectly orderly arrangement in closed chains, each of which may be composed of a large group of molecules of the substance. These molecules, when the substance is unmagnetised, form a closely coherent arrangement which is broken up into an entirely different collocation under the influence of an externally applied magnetic field. Thus if the wire of the unmagnetised material perform torsional oscillations, the relative motions of the parts of the material will naturally be affected by the internal forces existing between the parts, and by the arrangement of the parts in more or less distinct chains or groups. It is reasonable to suppose that the effect of magnetisation being to entirely alter these internal magnetic forces, and to break up and rearrange the groups of elementary magnets, will be also to alter considerably the internal friction shown by the material. With this idea in view, we have subjected wires of iron, nickel, and steel to longitudinal magnetising forces, while they were compelled to perform torsional oscillations round their axes, and have observed the rate of subsidence of these oscillations with magnetising forces of different values, the amounts of which will be found stated in curves illustrative of the results obtained. The effects of the fields were found to be considerable in amount and very curious in character. For example, in the case of iron, as the material was more intensely magnetised, the rate of subsidence of the oscillations continually diminished; on the other hand, with wires of nickel in the state in which they were received from the maker, the effect was to increase the rate of subsidence until a certain value of the magnetising force was reached, after which the rate of subsidence diminished with further increase of the magnetising force. These results were modified considerably by alteration in the state of the material, produced by drawing the wires through a draw-plate.

Since the completion of the experiments described in this paper, we have become aware of a paper by Mr. Herbert Tomlinson, on the subject of the effect of magnetisation on the internal viscosity of iron.* The results obtained by Mr. Tomlinson, show an increase of viscosity produced by a field of 35 units, which is contrary to the effects described below. But the extreme amplitude in Mr. Tomlinson's experiments was only 10° for a wire 1 metre long, as against 90° in our experiments; and as we have pointed out below, the effect seems to depend in an important way upon the amplitude, though

* 'Phil. Trans.,' A, vol. 179, 1888.

whether it changes sign in our specimens at very low amplitudes we are as yet unable to say.

It will be convenient to give here a short description of the apparatus, and then to state in detail the results obtained from the experiments made on the three substances referred to. The specimens were wires about 1 metre long, and about 1·3 mm. in diameter. The exact dimensions are given below where the particular specimens are referred to in the arrangement adopted. The wire is suspended vertically along the axis of a magnetising coil A (Diagram I). Across the upper end of the coil is placed a cross-piece of brass, to which the upper end of the wire is attached, while the coil itself is supported by the upper cross-bar of a Willis frame which stands on the laboratory

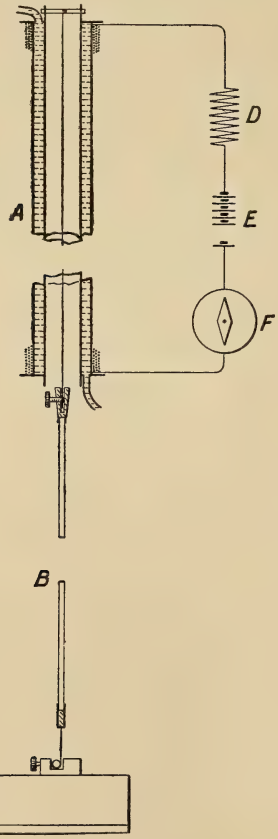


Diagram I.

floor. About 3 or 4 cm. below the lower end of the coil, the wire is attached to a brass fitting cemented on to the upper end of a glass

rod B about a metre long and 5 mm. in diameter. To the lower end of this glass rod is rigidly fastened a cylindrical vibrator C. The axes of the wire, the glass rod, and the vibrator are as nearly as possible in one vertical straight line, and the three bodies are rigidly connected, so that if the vibrator is turned round its axis, no slipping takes place at the attachments. The wire and rod are thus subjected to twist, the amount of which is infinitesimal in the glass rod, which turns round its axis practically as a rigid body; so that the whole twist may be regarded as contained in the wire. This arrangement was adopted to keep the vibrator, which in all cases was of iron (being selected from a series of vibrators in the collection of apparatus), sufficiently remote from the coil to obviate the possibility of any action between the field of the coil and the moving vibrator, while the whole of the twisting and untwisting material was contained within the field. Round the lower edge of the vibrator is gummed a millimetre scale, which is read by a telescope, and the vibrations were always sufficiently slow to allow of the scale being read at the beginning and end of a semivibration, the difference between the two readings giving the amplitude. The coil consisted of 3080 turns of copper wire wound on a double core of brass tubing. This double core was made of two coaxial tubes, the outer $1\frac{1}{2}$ inches and the inner $\frac{1}{2}$ inch in diameter. The space between the tubes forms a water-jacket, through which a stream of water can be forced so as to shield the wire from the heating effect of the current in the coil. The current produced by the battery E, and adjusted by the resistance D, was measured by a Kelvin graded galvanometer F, and the field was calculated from the value $4\pi Cn$ for the intensity of the field produced by a current of C absolute units flowing in a coil of n turns per cm. of length.

Results for Nickel.—The wire used was obtained from Messrs. Johnson and Matthey, London, and was stated by them to contain only a very small percentage of impurity. Its diameter was 1·4 mm. The results of the experiments are shown in Diagram II. In the curves of that diagram the abscissæ, drawn from left to right, show amplitude of vibration in degrees; the ordinates, drawn downwards, represent the number of periods which have elapsed from the beginning of the set of observations. Thus we obtain the amplitude left after different numbers of oscillations have been performed, and are able to estimate from the curve the rate of subsidence. The ordinates may be taken as giving the time from the commencement of the set of observations, as it was found that the alteration of period produced by the imposition of the field was negligible, the actual periods being 7·21 seconds with a field of 132 C.G.S. and 7·17 seconds with no field. It will be seen that the effect of small fields is to greatly increase the rate of subsidence, but that at a field of about 160 C.G.S. the maximum effect of the field in increasing the rate of subsidence is produced: thus

Curves 1, 2, 3, 4 for fields up to 156 C.G.S., lie successively farther to the left in the diagram, while the Curves 5, 6, 7, 8 for fields from 187 to 360 C.G.S. lie in succession to the right of one another, showing in the first case a continual increase, and in the second a continual diminution in the rate of subsidence.

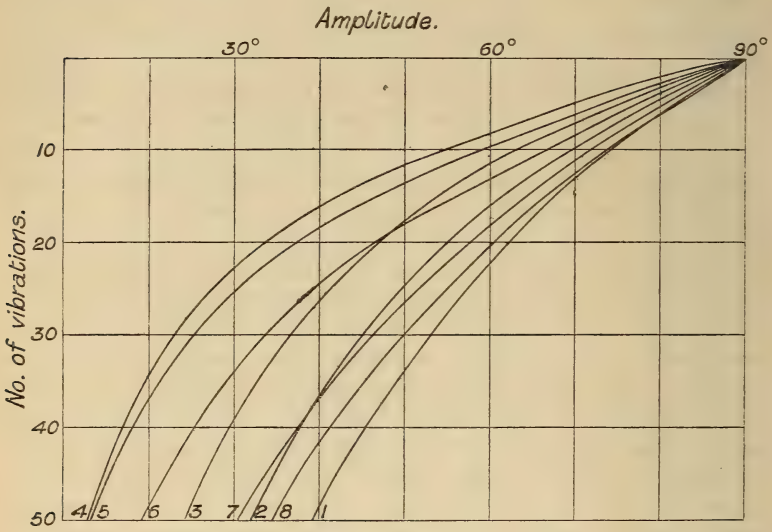


Diagram II.

Curve.	Field.	Curve.	Field.
1	0 C.G.S.	5	187 C.G.S.
2	22 ,,	6	257 ,,
3	50 ,,	7	330 ,,
4	156 ,,	8	360 ,,

A curve for zero field was taken after these eight curves had been obtained, and it was found to agree closely with Curve 1 of the series shown in the diagram.

The logarithmic decrements were calculated for these curves and were found to diminish in each curve as the amplitude diminished. This diminution was about half the initial value of the logarithmic decrement for each of the first four curves, but then fell off until in curve 8 it was only $\frac{1}{5}$ of the value at the initial amplitude.

A series of experiments was made for the purpose of finding more precisely the value of that magnetising field which had maximum effect upon the rate of subsidence, and curves of subsidence were obtained for values of the field ranging from about 100 to about 200 C.G.S. The curves form a close series, and intersect one another in a way which it is rather difficult to disentangle, and which renders the critical value of the magnetic field in a sense indefinite. The crossing of the curves in the immediate neighbourhood of the critical

field was traced to a slight extent to small uncompensated effects of heating, but it is extremely unlikely that the crossing of the curves is due to this cause alone, as no doubt the effect of the magnetic field on the rate of subsidence depends upon the amplitude. Further experiments with more nearly perfect prevention of heating effects, are required to enable the dependence of the rate of subsidence on amplitude to clearly disclose itself. It may be mentioned, however, that the actual variations in temperature, as determined by observations of the temperature of the water issuing from the jacket, were very slight. For example, the temperatures at the end of each of nine successive experiments—the first and last of which were made with no current in the coil, and the others with currents from 2·56 to 6 amperes, varied from 6°·5 C. to 8° C.

It will be noticed that in Diagram II the shape of the curves undergoes change as the magnetic field is increased. This is most obvious near the critical value of the field, as shown by Curves 4 and 5. For Curve 4 the rate of subsidence at first is distinctly greater than in Curve 5, but as the amplitude diminishes the two curves approach one another, so that while, after ten, twenty, and thirty vibrations there is distinctly more amplitude left in Curve 5 than in Curve 4, there is practically the same amplitude left after fifty vibrations, the curves being then on the point of crossing. This effect seems, as we have said, to be, in part at least, due to the dependence of the magnetic effect on amplitude.

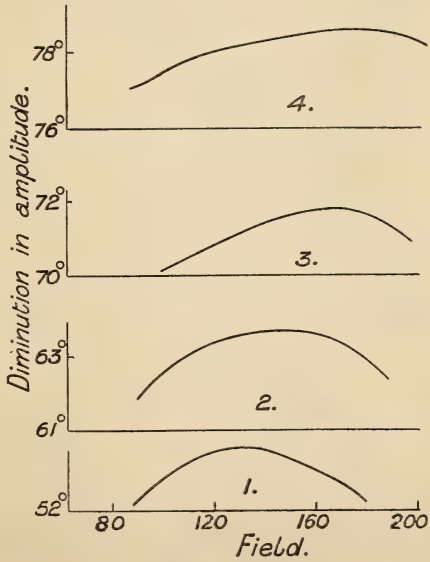


Diagram III.

Diagram III brings out the point just alluded to more clearly. These curves show the effect of the different fields upon the amount of subsidence effected in the times corresponding to different numbers of vibrations and to different amplitudes. For example, Curve 1 gives a comparison of the subsidences (from an initial amplitude of 90°) effected in twenty vibrations under different fields varying from about 100 to 200 C.G.S. Curve 2 gives a comparison of the subsidences from the same initial amplitudes effected in thirty vibrations for the same range of field. Curves 3 and 4 show the subsidences for forty-five and seventy-five vibrations respectively in the same circumstances. It will be seen that the points of maximum of the curves trend to the right from Curve 1 to Curve 4—that is to say, the value of the field which produces maximum effect is greater the smaller the amplitude of the vibration to which it is applied.

Results for Iron.—Diagram IV shows curves of subsidence obtained for iron. Six curves are given showing the results for fields varying

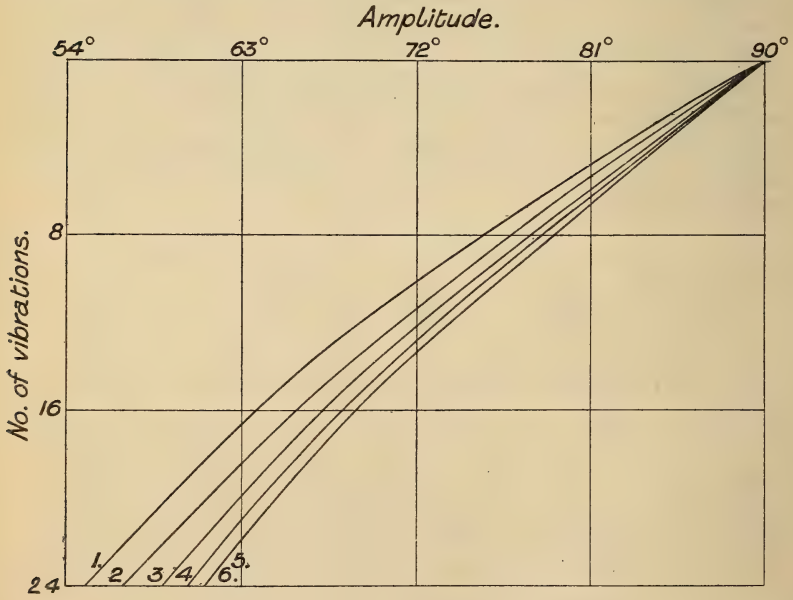


DIAGRAM IV.

Curve.	Field.	Curve.	Field.
1	0 C.G.S.	4	81 O.G.S.
2	22 ,,	5	170 ,,
3	44 ,,	6	229 ,,

from 0 to 230 C.G.S. It will be seen that the curves fall successively to the right of one another with the exception of 5 and 6, which are practically coincident. Experiments were also made for fields up to

about 400 C.G.S., but the curves obtained showed only a very slight tendency to move to the right, so slight that if they had been drawn they could not have been distinguished in the diagram from Curves 5 and 6. The same vibrator was used for the iron wire as had been used in the case of nickel, and the period of vibration was 5·27 seconds. For iron, then, we have the remarkable result that the effect of a magnetic field on the rate of subsidence of torsional oscillations is to diminish that rate, and by an amount diminishing with increasing field until a field of about 160 or 170 C.G.S. is reached, after which the effect of the field, however great, is practically constant. This is, of course, what one would expect in consequence of saturation of the iron, but it is a result very remarkably different from that obtained for nickel. The totally different behaviours in the two cases seem to point to an entirely different collocation of elementary magnets in the nickel and the iron, *i.e.*, perfectly distinct molecular constitution, consistent, however, in each case with magnetisability.

Results for Steel.—Diagram V shows three curves obtained for pianoforte steel wire. The same vibrator was employed as in the former

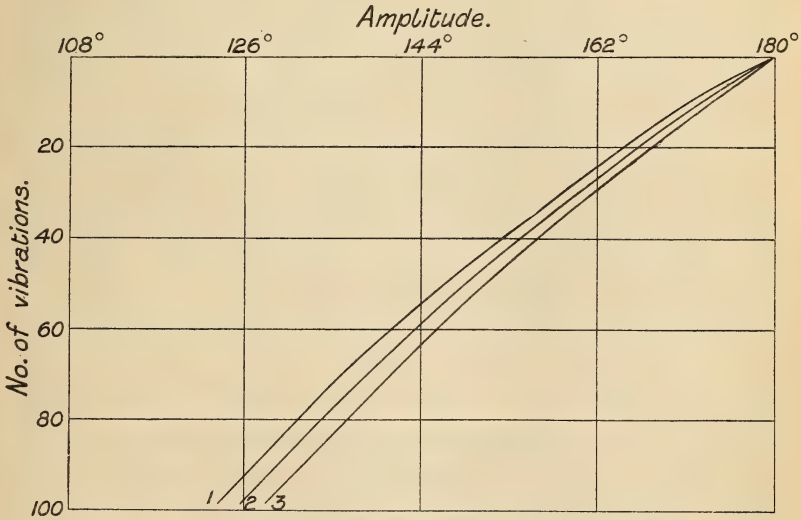


Diagram V.

Curve.	Field.
1	0 C.G.S.
2	110 „
3	230 „

cases, and it gave with this wire a period of 16·81 seconds. The fields were respectively 0, 110, and 230 C.G.S., and it will be noticed that the results are very similar to those obtained for iron, except that the

effects are much smaller. The curves for higher fields moved continually to the right, and showed no tendency to become coincident.

In both iron and steel the logarithmic decrements, as the curves show, were much more nearly constant throughout each curve than was the case for nickel. In steel the decrement fell off rapidly at first, so that throughout the greater part of the curve it was nearly constant at a distinctly smaller value than at the beginning of the curve.

Experiments were also made with a view to ascertaining whether the change in the physical properties of the wires produced by drawing them through a draw-plate altered in any way the effects of magnetisation on the rate of subsidence. In the case of drawn nickel wire the most striking alteration was the disappearance of the reversal of the effect of magnetisation. The magnetisation increased slightly the rate of subsidence, and this increase continued quite regular to the highest values of the field obtainable (about 400 C.G.S.), no critical value of the field similar to that observed for the undrawn wire being obtained. In the case of drawn iron, on the other hand, the effect of magnetisation was to regularly diminish the rate of subsidence. The effect was thus similar to that obtained for the undrawn wire, the chief point of difference being that, in the case of the drawn wire, further increase of the field gave farther perceptible diminution in the rate of subsidence, so that there was not that approximation to constancy of effect of field, apparently depending on magnetic saturation, which was observed in the case of the undrawn wire. The drawing of the wires was found to result in a considerable increase of their rigidity.

As a check upon the experiments detailed above, a copper wire was substituted for the iron or nickel wire along the axis of the coil, and a field of 230 C.G.S. units was applied. No perceptible effect upon the rate of subsidence was found to be produced.

[*Note, added May* 30, 1902.—The effect of annealing the nickel wire which had been hardened by drawing has now been observed. The wire was heated to a bright red heat and then suddenly cooled by being plunged into water. It was found that the wire had returned to its former state, as all the features of the curves of Diagram II were again observed, with the difference that the curve of maximum rate of subsidence occurred with a field of only about half the former value. Experiments on the effect of annealing the iron wires are in progress, and show that the effect of annealing, by heating and slow cooling, is to annul the change produced by drawing.]

'The Spectra of Potassium, Rubidium, and Cæsium, and their Mutual Relations." By HUGH RAMAGE, B.A., St. John's College, Cambridge. Communicated by Professor G. D. LIVEING, F.R.S. Received May 14,—Read June 5, 1902.

The spectra of this group of metals have already been considered in a paper by the author, on "A Comparative Study of Spectra," &c.* It was there shown that the differences between the principal series of lines in these metals depended on the atomic mass alone; and also that there was a close connection between the subordinate series and the atomic mass. A further study of the latter series was impossible at the time of writing the paper owing to the fewness of the lines which had been observed and measured in them; practically no lines were known in the second subordinate series of rubidium and cæsium.

Some lines belonging to the subordinate series have been measured in Bunsen-flame and spark spectra by Lecoq de Boisbaudran, and in the arc spectrum by Liveing and Dewar, and by Kayser and Runge. Lehmann has measured some lines in the arc spectra in the red region. Lines recorded by these observers were found by the writer, with considerable intensity, in the oxyhydrogen flame spectra of the metals; and other lines, weaker than the above, were present which had never been recorded. Photographs of these high-temperature flame spectra were taken with a spectrometer designed by Professor Liveing, fitted with a Rowland plane grating ruled with 14,438 lines to the inch. The quartz lenses were plano-convex with a focal length for the D lines of about 778 mm. The spectra in the first and second orders were photographed, and some measurements were made in the red region by eye observations. Spark spectra were photographed, superimposed on the flame spectra, of iron and titanium principally, but other metals were also employed. These furnished the numerous fiducial lines required for the accurate determination of the wave-lengths.

The lines in the subordinate series are generally more diffuse than those in the principal series. Some of the weaker lines, notably those of cæsium, are very broad with diffuse edges; very accurate measurement of these is impossible.

Particulars of the spectra are recorded below; the oscillation frequencies are reduced to their values in a vacuum. The lines have been sorted into the principal and the first and second subordinate series, and marked P, I, or II, with the number of the line, according to Rydberg's formula, in the sixth column. The wave-lengths of the lines which have been observed before are given in the fourth column.

* 'Roy. Soc. Proc.,' vol. 70, p. 1, 1902.

In the column of observers, L. and D. represent Liveing and Dewar, K. and R., Kayser and Runge; L. de B., Lecoq de Boisbaudran.

Cæsium

Wave-length.	Oscillation frequency, in vacuum.	Intensity.	Previous measurement.	Observer.	Series and number.
6984	14314	6			
74	335	9	6973·9	K. and R.	I_4
6869	554	2			
29	639	2			
6722	873	9	6723·6	,,	I_4
6630	15079	2			
6590	171	8	—	—	II_4
6472	447	2			
6433	540	2			
6354	733	8	—	—	II_4
6217·6	16078·7	2			
13·33	89·7	8	6213·4	K. and R.	I_5
6034·43	16566·7	4	—	—	II_5
10·59	16632·4	8	6010·6	K. and R.	I_5
5847·86	17095·6	2			
45·31	102·7	8	5845·1	,,	I_6
39·33	120·2	2	—	—	II_5
5746·37	397·2	1	—	—	II_6
5664·14	649·7	7	5664·0	K. and R.	I_6
35·44	739·6	5	35·1	,,	I_7
5574·4	933·9	1	5572	L. de B.	II_7
68·9	951·6	1	—	—	II_6
03·1	18166·2	3	5501·9?	K. and R.	I_8
5466·1	289·2	4	5465·8	,,	I_7
14·4	463·8	1	—	—	I_9
07·5	487·3	1	—	—	II_7
5351	682	1	—	—	I_{10}
5341·15	717·0	3	5345	L. de B.	I_8
5304	848	<1	—	—	I_{11}
5256·96	19016·8	1	5257	L. de B.	I_9
5209	192	<1			
5199	228	<1	—	—	I_{10}
5154	396	<1	—	—	I_{11}
4593·30	21764·8	8	4593·34	K. and R.	P_2
55·46	945·6	10	55·44	,,	P_2
3888·75	25707·9	2	3888·83	,,	P_3
76·31	790·4	4	76·73	,,	P_3
3617·49	27635·7	<1	3617·08	,,	P_4
11·70	680·0	2	11·84	,,	P_4
3477·25	28750·3	1	—	—	P_5
3398·40	29417·3	1	—	—	P_6
48·72	29853·7	<1	—	—	P_7
3314	30166	<1	—	—	P_8
3287	30414	<1	—	—	P_9

Rubidium

Wave-length.	Oscillation frequency, in vacuum.	Intensity.	Previous measurement.	Observer.	Series and number.
			7950·46	Lehmann	P_1
7799	—	Very strong	7805·98	,,	P_1
6306·8	15851·3	1			
6299·19	870·5	9	6297	L. de B.	I_4
6206·74	16106·8	8	6203	,,	I_4
6160·04	228·9	5	6159	,,	II_4
6071·04	466·8	4	—	—	II_4
5724·62	17463·2	8	5724·41	K. and R.	I_5
5654·16	680·9	3	5654·22	,,	II_5
48·19	699·6	7	48·18	,,	I_5
5579·3	918·1	2	—	—	II_5
5432·05	18403·9	6	5431·83	K. and R.	I_6
5391·3	543·0	1	—	—	II_6
63·15	640·3	5	5362·94	K. and R.	I_6
22·83	781·5	1	—	—	II_6
5260·51	19004·0	4	5259·8	K. and R.	I_7
34·6	098	1	—	—	II_7
5195·76	240·7	3	5194·8	K. and R.	I_7
65·35	354·1	2			
51·20	407·2	2	—	—	I_8
5132	480	<1	—	—	II_8
5089·5	642·5	1	—	—	I_8
76·3	693·6	1	—	—	I_9
37	847	1			
23	902	1	5021·8	K. and R.	I_{10}
17	926	<1	—	—	I_9
4983	20062	<1	—	—	I_{11}
67	127	<1			
4215·68	23714·4	9	4215·72	K. and R.	P_2
02·04	791·4	10	01·98	,,	P_2
3591·86	27832·8	3	3591·74	,,	P_3
87·27	868·4	4	87·23	,,	P_3
3350·98	29833·5	1	3351·03	,,	P_4
48·84	852·6	2	48·86	,,	P_4
3229·26	30958·0	1	—	—	P_5
28·18	968·4	1	—	—	P_5

The isolated line λ 5165·35 is narrow and sharp; it differs, in these respects, from the lines in the series.

Potassium

Wave-length.	Oscillation frequency, in vacuum.	Intensity.	Previous measurement.	Observer.	Series and number.
7697	—	Very strong	7701·92	Lehmann	P_1
7664	—	,,	7668·54	,,	P_1
6939	14407	8	6938·8	K. and R.	II_3
13	462	7	11·2	,,	II_3
5832·25	17141·3	6	5832·23	,,	I_4
12·53	199·5	5	12·54	,,	I_4
02·12	230·8	7	02·01	,,	II_4
5782·74	288·1	6	5782·67	,,	II_4
5359·96	18651·8	4	5359·88	,,	I_5
43·38	709·6	2½	43·35	,,	I_5
40·17	720·9	3	40·08	,,	II_5
23·68	778·9	2	23·55	,,	II_5
5112·76	19553·1	2	5112·68	,,	I_6
5099·83	602·7	1	5099·64	,,	II_6
97·64	611·1	1½	97·75	,,	I_6
85·07	659·4	1	84·49	,,	II_6
4965·61	20132·5	1	4965·5	,,	I_7
4957	167	<1	56·8	,,	II_7
51·46	190·1	1	52·2	,,	I_7
4870	528	<1	4870·8	L. and D.	I_8
62	562	<1	63·8	,,	II_8
57	583	<1	56·8	,,	I_8
29	702	<1			
03	814	<1	4803·8	,,	I_9
01	823	<1	—	—	II_9
4798	836	<1	4796·8	L. and D.	
67	972	<1			
60	21002	<1	4759·8	,,	I_{10}
4642·35	21534·4	2	4642	H. and R.	
38·6	51·8	<1			
4047·39	24700·3	9	4047·36	K. and R.	P_2
44·33	719·0	10	44·29	,,	P_2
3447·56	28997·8	3	3447·49	,,	P_3
6·55	29006 3	4	6·49	,,	P_3
Present	—	<1	3217·76	,,	P_4
3217·36	31072·7	2	0·27	,,	P_4

Other lines, very feeble indeed, appear on the strong continuous spectrum in the region near 4642. The line λ 4642·35 was first observed in the spectrum of the Bessemer flame; Hartley and Ramage, 'Phil. Trans.,' A, vol. 196, p. 491, 1901.

Diagrams of these spectra were drawn, as described in my former paper, to scales of oscillation frequencies for abscissæ, and (1) atomic masses, (2) squares of atomic masses for ordinates. The conclusions previously deduced from the less complete data were thereby amply confirmed. There is undoubtedly a very close connection between the spectra and the atomic masses; and the lines, which connect the corresponding members of homologous doublets in diagram (2), do intersect on the line of zero atomic mass.

The two limits in each spectrum towards which the two subordinate series appeared to converge were determined by a slight modification of Rydberg's method combined with graphical methods. These were inserted in the diagrams and curves were drawn through the points. In diagram (1) the curves were turned away from each other and the points of bisection of the lines between the limits lay on a straight line; so also did the points of bisection of the lines between the two more refrangible and corresponding doublets of the second subordinate series. In diagram (2) the curves through the limits of the series, when produced, intersected on the line of zero atomic mass. This fact indicates that the difference between the two limits of the series, while not proportional to the square of the atomic mass, is a simple function of it. Rydberg, Kayser and Runge, and Rummel* have each shown that the differences between the convergence points of the subordinate series are approximately proportional to the squares of the atomic masses.

A diagram of the spectra and limits of the series was also drawn for the three metals to scales of wave-lengths and atomic masses. The more refrangible limits of the subordinate series and the more refrangible members of the second series now lay on straight lines; the change in wave-length was thus proportional to the atomic mass.

After a careful study of the facts and many computations, it was found possible to calculate the subordinate series with considerable accuracy by the following formulæ.

The first Subordinate Series.

The two convergence points (n_∞) of this series are obtained as follows: $n_\infty = 22830 - 21{\cdot}633\,\mathrm{W} \pm \frac{\mathrm{A}}{2}$, where W is atomic mass and A is the average difference between the doublets. The latter quantity, as determined from the lines which are best suited for accurate measurement, is for potassium 57·8, for rubidium 236·4, and for cæsium 547·6. These values, as shown above, are simple functions of the atomic mass; but the best method of expressing them is not yet clear. This formula gives the following values for n_∞ belonging to the doublets of the first subordinate series:—Potassium, 21953·9 and 22011·7; rubidium, 20861·8 and 21098·2; and cæsium 19677·2 and 20224·8.

When these values are substituted for n_∞ in Rydberg's formula,

$$n = n_\infty - \frac{\mathrm{N}_0}{(m+\mu)^2};$$

in which $n = 10^8\lambda^{-1}$, $\mathrm{N}_0 = 109675$, $m = 3 . 4 . 5 \ldots$ and when we

* 'Proc. Roy. Soc. Victoria,' vols. 9 and 10, 1897.

also substitute for μ (assuming it to have a constant value for the series) the value

$$\mu = 0 \cdot 7869 - 1466\ W^2 \times 10^{-8},$$

we obtain the following results:—

Potassium

m.	Oscillation frequencies.		Differences.
	Observed.	Calculated.	
3	—	14214·6	
4	17141·3	17122·4	−18·9
5	18651·8	18653·3	+ 1·5
6	19553·1	19557·1	+ 4·0
7	20132·5	20134·7	+ 2·2
8	20528	20526·1	− 1·9
9	22814	20803·6	−10·4
10	21002	21007·4	− 5·4

m.	Oscillation frequencies.		Differences.
	Observed.	Calculated.	
3	—	14272·4	
4	17199·5	17180·2	−19·3
5	18709·6	18711·1	+ 1·5
6	19611·1	19614·9	+ 3·8
7	20190·1	20192·5	+ 2·4
8	20583	20583·9	+ 0·9
9			
10			

Rubidium

m.	Oscillation frequencies.		Differences.
	Observed.	Calculated.	
3	—	12763·2	
4	15870·5	15854·4	−16·1
5	17463·2	17462·3	− 0·9
6	18403·9	18404·0	+ 0·1
7	19004·0	19002·3	− 1·7
8	19407·2	19406·1	− 1·1
9	19693·6	19691·3	− 2·3
10	19902	19900·3	− 1·7
11	20062	20057·9	− 4·1

m.	Oscillation frequencies.		Differences.
	Observed.	Calculated.	
3	—	12999·6	
4	16106·8	16090·8	−16·0
5	17699·6	17698·7	− 0·9
6	18640·3	18640·4	+ 0·1
7	19240·7	19238·7	− 2·0
8	19642·5	19642·5	0
9	19926	19927·7	+ 1·7

Cæsium

m.	Oscillation frequencies.		Differences.
	Observed.	Calculated.	
3	10852·1	10865·7	+ 13·6
4	14335	14327·9	− 7·1
5	16089·7	16088·2	− 1·5
6	17102·7	17103·6	+ 0·9
7	17739·6	17741·9	+ 2·3
8	18166·2	18169·2	+ 3·0
9	18463·8	18469·1	+ 5·3
10	18682	18687·7	+ 5·7
11	18848	18851·9	+ 3·9

m.	Oscillation frequencies.		Differences.
	Observed.	Calculated.	
3	11404·1	11413·3	+9·2
4	14873	14875·5	+2·5
5	16632·4	16635·8	+3·4
6	17649·7	17651·2	+1·5
7	18289·2	18289·5	+0·3
8	18717·0	18716·8	−0·2
9	19016·8	19016·7	−0·1
10	19228	19235·3	+7·3
11	19396	19399·5	+3·5

The second Subordinate Series.

In this series

$$n_\infty = 22850 - 21 \cdot 812\ W \pm \frac{B}{2},$$

where B is for potassium, 57·8; for rubidium, 238·0; and for cæsium, 553·6; and

$$\mu = 0 \cdot 7990 + 7984\ W^2 \times 10^{-9}.$$

It will be observed that the doublets in the second subordinate series are more widely separated than those in the first series. It would appear also that the two series do not converge towards the same limit; the difference between the limits, however, diminishes in the different metals as the atomic mass increases. This is true on the supposition that μ is constant, and not variable as in the formula given for the principal series. Kayser and Runge hold the view that there are different limits for the two series, while both Rydberg and Rummel favour the view that the limits are the same.

The observed and calculated oscillation frequencies are as follows :—

Potassium

m.	Oscillation frequencies.		Differences.
	Observed.	Calculated.	
3	14407	14417·4	+10·4
4	17230·8	17229·9	− 0·9
5	18720·9	18720·3	− 0·6
6	19602·7	19603·9	+ 1·2
7	20167	20170·5	+ 3·5
8	20562	20555·3	− 6·7
9	20823	20828·6	+ 5·6

m.	Oscillation frequencies.		Differences.
	Observed.	Calculated.	
3	14462	14475·2	+13·2
4	17288·1	17287·7	− 0·4
5	18778·9	18778·1	− 0·8
6	19659·4	19661·7	+ 2·3

Rubidium

m.	Oscillation frequencies.		Differences.
	Observed.	Calculated.	
3	13498	13496·7	−1·3
4	16228·9	16219·6	−9·3
5	17680·9	17671·4	−9·5
6	18543·0	18535·8	−7·2
7	19098	19091·8	−6·2
8	19480	19470·3	−9·7

m.	Oscillation frequencies.		Differences.
	Observed.	Calculated.	
3	13738	13734·7	−3·3
4	16466·8	16457·6	−9·2
5	17918·1	17909·4	−8·7
6	18781·5	18773·8	−7·7

Cæsium

m.	Oscillation frequencies.		Differences.
	Observed.	Calculated.	
3	—	12609·3	
4	15171	15180·2	+9·2
5	16566·7	16566·1	−0·6
6	17397·2	17397·3	+0·1
7	17933·9	17934·7	+0·8

m.	Oscillation frequencies.		Differences.
	Observed.	Calculated.	
3	—	13162·9	
4	15733	15733·8	+0·8
5	17120·2	17119·7	−0·5
6	17951·6	17950·9	−0·7
7	18487·3	18488·3	+1·0

The convergence points of the series as deduced in different ways are given in the following table :—

Element.	From above formulæ.			By calculation from observed lines.	From formula for principal series.*	Numbers calculated by Rydberg.
	First series.	Second series.	Mean of two series.			
				Mean.		
Potassium (1)	21953·9	21968·0	21960·95	21960	21969·4	21955·46
,, (2)	22011·7	22025·8	22018·75	22018	22024·3	22013·31
Rubidium (1)	20861·8	20868·3	20865·65	20865	20868·6	20869·15
,, (2)	21098·2	21106·3	21102·25	22101	21112·3	21098·83
Cæsium (1)	19677·2	19674·2	19675·7	19672	19686·7	
,, (2)	20224·8	20228·0	20226·4	20226	20234·2	

The numbers in the sixth column were obtained by the law, discovered by Rydberg and independently by Schuster, which connects the principal and subordinate series : the convergence points of the subordinate series are given by the differences between the convergence points and first lines (for which $m = 1$) of the principal series. One set of the numbers was obtained from the expression

$$\frac{N_0}{(1+1\cdot19126+0\cdot00103\,W)^2},$$

and the other set from the expression

$$\frac{N_0}{(1+1\cdot19126+0\cdot00103\,W+182\,W^2\times10^{-8})^2}.$$

The figures in this column agree best with those of the second subordinate series in the third column ; and it will be remarked as confirming the closer connection between the principal and second subordinate series, that the results calculated for the latter series of rubidium differ by about nine units, whereas those given by the formula for the principal series differ by about 27·5 units from the observed numbers. The connection between the first subordinate series and the atomic mass is apparently simpler than between the other two series and the atomic mass.

The numbers in the last column were taken from Rydberg's paper.† He calculated them by means of an empirical formula, from the observed lines.

All the strong lines, and nearly all the weak lines which have been observed in the flame and arc spectra of these elements, are included in the three harmonic series of lines. The empirical formulæ given show that the differences in the corresponding series depend wholly on the atomic masses of the three elements.

* Author, *loc. cit.*

† 'Paris Congress Reports,' vol. 2, p. 212, 1900.

"Contributions to the Study of Flicker. Paper II." By T. C. PORTER, M.A., Eton College. Communicated by LORD RAYLEIGH, F.R.S. Received May 14,—Read June 5, 1902.

In the first paper,* curves are given which show the rate at which a disc half white half black must be rotated in order that flicker may just vanish when the disc is illuminated by the different colours of a diffraction spectrum. Also curves showing how the rate of rotation alters with the alteration in the angular width of the coloured sector when placed in the different colours of the same spectrum. These last are reproduced with some additions and corrections in the present paper (fig. 4). It was shown in that paper (*a*) that as the stimulus applied to the retina increases in intensity, the impression produced retains its maximum value for a shorter and shorter time. (*b*) that a stimulus requires a finite time to produce its maximum effect. (*c*) that the duration of the impression on the retina *undiminished* decreases as the time during which the stimulus is applied increases, one of these quantities being roughly inversely proportional to the other.

There were also certain conclusions which confirm the work of others, and which it seems unnecessary to repeat. Since this first paper was written, continued experiments have suggested fresh means and precautions for making the observations still more accurate, and these must be first described.

(1.) The quantity of white light reflected from the black sector hitherto used was measured, by comparing its luminosity with that of a disc having two narrow white tapering projections, subtending known angles at the centre. This was made to rotate, and viewed when flickerless, against a pitch dark background. It was thus proved that the quantity of white light reflected by the Indian ink with which the black part of the disc was painted varied, when the disc was regarded at different angles, from 4 per cent. to 10 per cent. of that reflected by the white cardboard used, and although in the experiments already described, the disc had always been regarded from very nearly the same standpoint, when it reflected 7 per cent., nevertheless it seemed better to use a black pigment which would diffuse what light it reflected more evenly. The disc was therefore coated with a mixture of lampblack and alcohol, shellac varnish being added in just sufficient quantity to prevent the lampblack from rubbing off. On careful measurement by the method already described, the quantity of white light reflected from this very "dead" black proved sensibly constant for a comparatively large variation in the angle of reflection, and equal to 4·1 per cent.

It seems well at this point to state how the want of blackness in

* 'Roy. Soc. Proc.,' vol. 63, p. 347.

the black sector affects the flicker curves (such as those given in fig. 3 of the present paper), and perhaps the simplest way to make this plain, is to take a numerical example: suppose then that the amount of white light reflected from 360° of black is equal to that reflected from 24° of the white cardboard from which the disc is made; then, if the angular breadth of the white sector is, say, 60°, the total amount of white light reflected from the remaining 300° of black will be equal to the light reflected from 22° of white, and this light is distributed evenly throughout the black.

Now if this disc is made to rotate until it becomes flickerless, its luminosity or albedo will be rightly expressed as that of a mixture of 60° + 22° = 82° of white, and 278° of absolute black, and if a second disc were made with a white sector of 82°, and an absolutely black sector of 278°, this second disc, when flickerless, would appear of the same brightness as the other disc of 60° white, and 300° imperfect black, but—and this is the point of the illustration—the flicker does not just vanish at the same speed of rotation for the two discs,—for flicker depends essentially upon *contrast*, and the contrast between the white and absolute black is greater than between the same white and imperfect black. Let the whiteness or albedo of the white cardboard be expressed numerically by 100, then the flicker in the case of the first disc is produced by the contrast between the 60° sector of white of albedo 100, and the 300° sector of what is really dark grey, albedo 7·3. Thus 7·3 of the white sector's brightness will contribute nothing to the flicker, for the disc may be regarded as being of the albedo 7·3 all over, there being added, where the white sector is, an additional albedo, not of 100, but of 100 – 7·3 = 92·7. In order, therefore, that flicker may vanish on a disc of 60° white, and 300° of absolute black, at the same rate of rotation as for a disc of 60° white and 300° of the imperfect black already mentioned, we must lessen the contrast between the white and black of the first disc by reducing the albedo of its white sector in the ratio of 92·7 : 100, and this can be done experimentally by altering the distance of the rotating sector from the source of light. Now the effect of altering the distance of the disc from the source of illumination is shown by the curves of fig. 4, and since the greater the distance from the source of illumination the further the curve lies to the left in this figure, we see that the effect of the imperfection of blackness in the black sector of the rotating disc is to shift its curve further to the left than it would lie if the black were perfect, and thus, as is evident from the figure, to make it of a rather flatter form than it would otherwise be: it is also evident that unless the amount of light reflected by the black sector is large, the shift in the curve is small, and the difference in form very slight.

(2.) The rate of rotation at which flicker appears just to vanish, depends, amongst other things, on the distance of the observer from

the rotating disc; the greater this is, the more difficult is it to see the flicker, hence in the experiments recounted in this paper, the disc was viewed at the same distance. The distance chosen was that of most distinct vision, and though constant for each observer, differed slightly for different observers.

(3.) A black velvet mask was worn, both to avoid any possible complication produced by light reflected from the disc on to the observer's face, and thence either into the observer's eye obliquely, or back again on to the disc.

(4.) To determine the exact moment when all trace of flicker has vanished requires a considerable amount of practice: but is made far easier than it would otherwise be by the fact that flicker is much more easily seen by averted vision than by direct, so that if the eye be fixed upon the axis of the rotating disc, the flicker will be seen to disappear in the region immediately around the axis before it disappears from the parts of the disc near its circumference; with practice this makes it possible to time the appearance of the flicker with very fair accuracy.

It seems a somewhat remarkable fact that although the perception of colour and form so rapidly deteriorate in passing from the fovea centralis towards the peripheral portions of the retina, the retina's sensitiveness to flicker should, on the contrary, increase. Inasmuch as the perception of any motion is analogous to the perception of flicker, for in both cases the perception is caused by a change in the stimulus of certain retinal elements, it follows that the retina is increasingly sensitive to the motion of objects as their images on the retina lie nearer to its periphery. May not this be due to the fact that it has always concerned mankind to notice *motion* before any other quality in matter, and especially motion of objects near the limit of his field of view? When once this motion has been perceived, the man turns his eye so that the image of the moving object is brought to the fovea, where its colour and form can be appreciated; it seems, therefore, a natural arrangement that the attention of an animal should, in the first instance, be arrested by the sensitiveness of the retina to the motion of an object which is not the object of direct vision at the moment its motion is perceived.

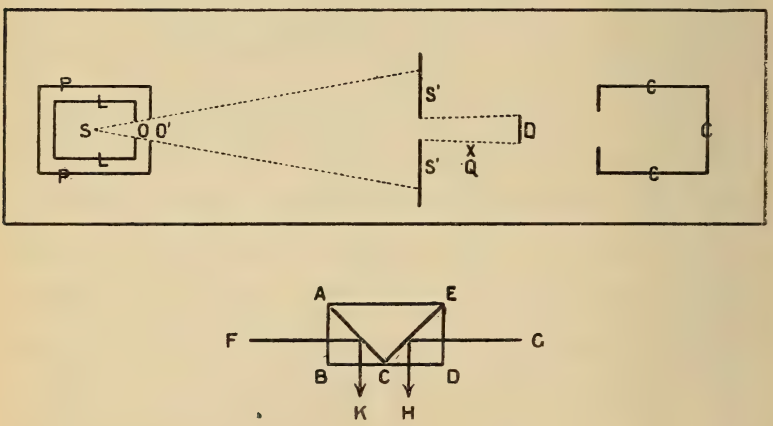
(5.) In timing the disappearance of flicker, it is not only a good plan to fix the eye on the axis of the disc, as has already been said, but it is necessary to keep the gaze steadily fixed on the axis, for when flicker has just disappeared to direct and steady gazing, the smallest movement of the eye, either obliquely across, or round the disc in the direction of rotation will reveal the flicker momentarily, or for as long as the motion of the eye lasts. The reason for the reappearance of the flicker scarcely requires explanation; it is evident that any motion of the eye such as that mentioned above will retard the rate of

passage of the image of the white and black sectors of the disc over the points of the retina, and will be equivalent, so far as the effect upon flicker is concerned, to making the disc rotate more slowly. There are two kinds of motion the eye may make without causing the flicker to reappear—the one radial, the other round the disc, in the opposite sense to that in which it is rotating. Experiment verifies these conclusions, though in practice the radial movement is not very easy to accomplish.

A set of experiments was then made, paying due regard to all the foregoing precautions, *to determine the exact number of revolutions of a disc half white, half black, at which flicker just vanishes, when the rotating disc is differently illuminated.*

The experimental arrangements can best be understood from fig. 1.

Fig. 1.

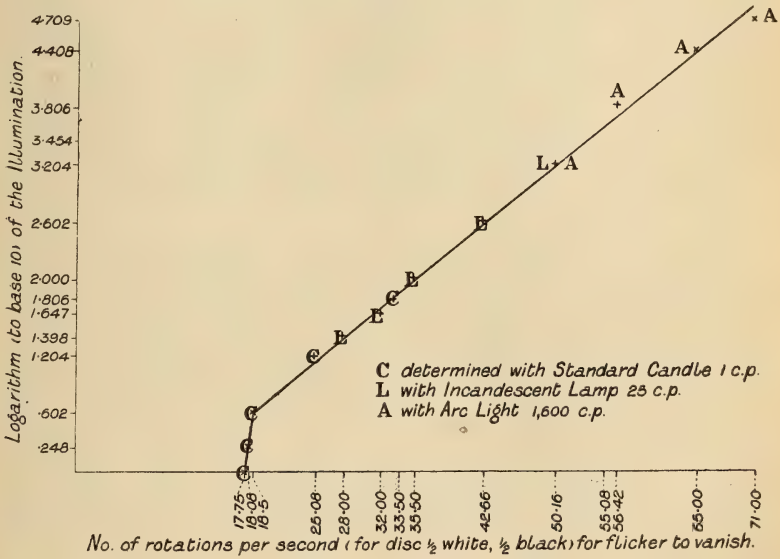


S is the source of the illumination of the disc D: this illumination can be changed in two ways, viz., either by altering the distance of D from S, or by changing the illuminating power of S itself. The length of the bench was more than 5 metres, and the illumination of the disc varied from that given by a sperm candle burning 8·273 grammes, or 127·67 grains per hour (which is meant when 1 candle-power is mentioned in this paper), at a distance from the disc of 4 metres, to the illumination given by an arc light of approximately 1600 ($\pm$ 50) candle-power at a distance from the disc of $\frac{1}{2}$ a metre.

Returning to the figure, LL PP are inner and outer boxes, painted with dead black inside, having openings at O and O′, with sharp edges, carefully blackened, wide enough to allow the whole of the illuminant to be seen from every part of the disc, when the latter was at its nearest to the illuminant. The screens PP LL kept the light from S from falling on the walls and ceiling of the room, and care was taken

that S was sufficiently raised above the working bench to prevent any rays of light from falling on the bench between S and D, and so getting reflected on to the disc or about the room. By several screens such as SS′ the stray light was further confined, with so good a result that, when it was necessary to bow one of the standard forks used to determine the pitch of the note given by the twelve syren holes in the rim of the disc, it was done chiefly by *feeling*, the forks being arranged in the order of their pitch on a neighbouring table and generally in charge of an assistant. Q in the figure is a small screen with two eye-holes through which the observer looks at the disc, and which can be arranged at any desired distance from D; some blackened screens,

Fig. 2.



CCC, receive any stray rays after they have passed D, so that the room may be described as very nearly dark (when the air is not unusually dusty, for of course any dust in the path of the rays scatters some light). The disc D is mounted on an electric motor, running smoothly, and since some of the sources of light used were electrical, it is important to state that the battery running the motor was altogether separate and independent of that which supplied the source of light. It is also necessary to give some account of the means by which the powers of the illuminants employed were gauged, the range of illuminations being unusually great. The photometer is shown in fig. 1; ABC and EDC are two right-angled prisms, having their faces BC and CD in one plane; both these faces are finely and evenly ground: light from the two sources is incident normally on the

faces AB, ED, and passing through the glass of the two prisms is totally reflected from their hypotenuses AC and EC, then falling on the ground-glass surfaces BC and CD. The prism and the sources of light are so adjusted that the albedos of these faces are equal when the brightnesses of the two sources are as the squares of their distances from C. First, then, for the lowest degrees of illumination, the sperm candle already mentioned was used. This was next replaced by an incandescent electric lamp, of the common type, supplied by a measured and constant current. This gave, as a mean of five determinations, the candle-power 25·06. A large coal-gas bat's-wing burner was then measured for candle-power against this incandescent lamp, and, in turn, this gas-burner was balanced photometrically against the arc light which was used for the brightest illuminations, and which gave as the mean of three very careful determinations 1600 candle-power with a possible error of 50 candle-power, though the probable error did not in the writer's estimation exceed 20 candle-power, if as much. The gas-burner was used to connect the measurements of the arc and 25·06 candle-power lamp, because the arc and this lamp depended on the same battery of accumulators, and hence could not be compared directly.

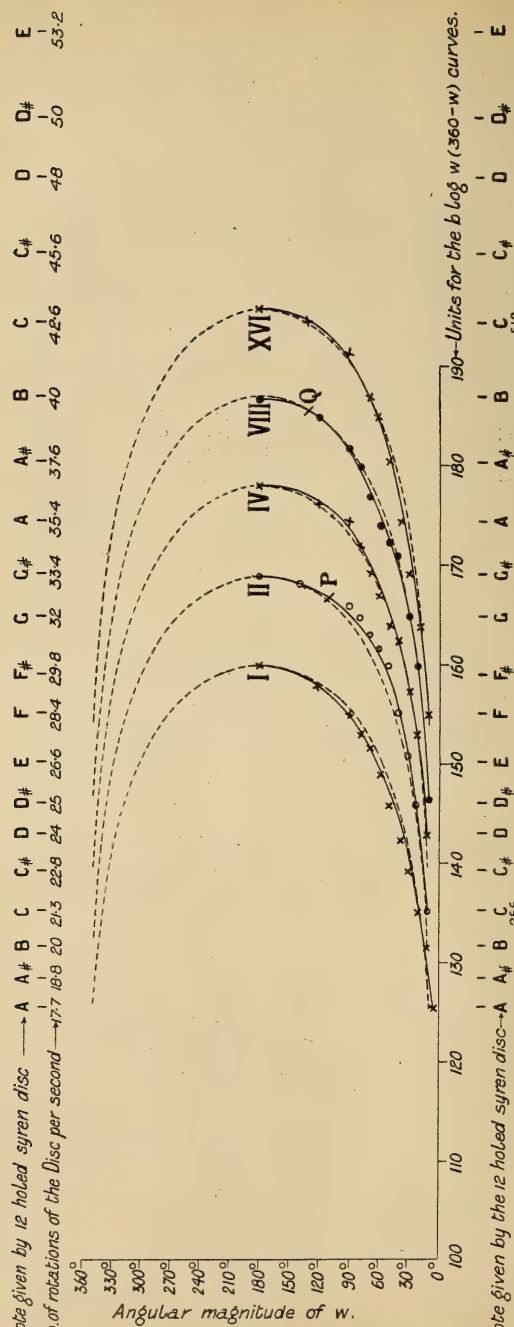
The results of this part of the investigation are exhibited tabularly below,* and in fig. 3 by curves. On the axis of Y distances are measured proportional to the *logarithms* of the different illuminations of the disc, half white half black, whilst on the axis of X can be read off the corresponding number of revolutions of the disc per second in order that flicker may just vanish, and it is at once evident that for all the illuminations between that caused by one candle at 2 metres and 12,800 times this illumination, *the speed with which the disc must be driven in order that flicker may just vanish varies directly with the logarithm of the illumination of the disc.* The same logarithmic law seems to hold good for illuminations smaller than these, but the rate of variation is different though constant.

With regard to the apparently sudden break in the line, it may at once be said that it may in reality be only a rather sharp curve, but that there is an unexpectedly rapid change in the direction of the line the writer feels certain. Each experiment was repeated at least three times for each observer (of which there were never less than two, and often as many as four), and, moreover, the whole set of experiments were repeated after an interval of 4 years.

* For this table, see next page.

Source of light used.	Distance of disc from source of light in metres.	Degree of illumination of disc.	Note given by 12 holes in the rim of the disc.	Number of sound vibrations per second.	Number of revolutions of disc per second.	Log. of illumination.
A standard candle.	4	1·0	A below C_{256}	213	17·75	0·000
	3	1·77	A below C_{256} about 4 vibrations per second sharp	217	18·08	0·248
	2	4·0	⅓rd of a tone above A.....	222	18·50	0·602
	1	16·0	D♯ above C_{256}	301	25·08	1·204
	0·5	64·0	A♭ above C_{256}	402	33·50	1·806
An incandescent electric lamp (Robertson's) giving 25 candle-power.	4	25·0	F above C_{256} a *very* little ♭	336	28·00	1·398
	3	44·4	G above C_{256}.....	384	32·00	1·647
	2	100·0	A above C_{256}..	426	35·50	2·000
	1	400·0	C_{512}	512	42·66	2·602
	0·5	1600·0	D♯ above C_{512}	602	50·16	3·204
Arc light of 1600 (±50) candle-power.	4	1600·0	D♯ above C_{512}, very little ♯	610	50·83	3·204
	3	2844·0	Mean between E and F above C_{512}	661	55·08	3·454
	2	6400·0	F about 5 vibrations per second *flat*	677	56·42	3·806
	1	25600·0	G a *very* little ♯	780	65·00	4·408
	0·5	51200·0	A above C_{512}.....	852	71·00	4·709

FIG. 3.


Note given by 12 holed syren disc ⟶ A A# B C C# D D# E F F# G G# A A# B C C# D D# E
No. of rotations of the Disc per second ⟶ 17·7 18·8 20 21·3 22·8 24 25 26·6 28·4 29·8 32 33·4 35·4 37·6 40 42·6 45·6 48 50 53·2
Angular magnitude of w.
360° 330° 300° 270° 240° 210° 180° 150° 120° 90° 60° 30° 0°
100 110 120 130 140 150 160 170 180 190—Units for the b log w (360−w) curves.
I II IV VIII XVI
P Q
Note given by the 12 holed syren disc→A A# B C C# D D# E F F# G G# A A# B C C# D D# E
256 512

Calling the illumination for which the direction of the straight line seems to change, the critical illumination, the equation to the upper part may be written $dn/d \log I = \cot 39°$ (approx.) for the scale of the diagram, whilst below the critical illumination it is $dn/d \log I = \cot 82° 30'$ (approx.). It may be noticed that the angle made with the axis of X by the lower part, *i.e.*, 82° 30′, is a little more than twice the similar angle made by the upper part, *i.e.*, 39°. Remembering that the scale of the logs (to base 10) is that of one-tenth, that of the numbers expressing the rotations per second, the two equations may be written—

$$n = 12 \cdot 4 \log I + 10,$$

and

$$n = 1 \cdot 56 \log I + 17 \cdot 75,$$

where n is the number of rotations of the disc per second when flicker just vanishes, and I is the illumination of the disc.

The actual "last" (undiminished) of the impression on the retina is obtained for any illumination as the fraction of a second by dividing unity by twice n. Thus, for the feeblest illumination experimented with, that of the standard candle at 4 metres, it is about 1/35th, whilst for the brightest illumination, that of the arc light at 50 cm., it is 1/147th. It is also to be noted that the illumination caused by the 25 candle-power lamp at 4 metres is less bright than that of the single candle at half a metre, and also that that of the arc light at 4 metres is as bright as that of the 25 candle-power lamp at half a metre, so that the illuminations given by the moderately bright source of light overlap, as it were, those given by the most feeble and the most intense, and the fact that the values of n obtained were equal when the photometric results showed that they should be so, is a proof that the errors made in estimating the quantitative values of the illuminants were not seriously in error. Direct photometric comparisons were made between the different illuminants both before and after each set of experiments, and it was thus proved that their illuminating powers had not sensibly changed. The lowest degree of illumination was one in which, in popular language, it was not easy to see, whilst the brightest was so bright as to make the disc uncomfortable to look at, even when the room in which the experiments were performed was flooded with the diffused light of a bright summer day.

Although the primary object of this research is to throw light upon the process of vision, there are some practical issues to the lines of fig. 3. We can easily determine by its means the number of pictures which must be projected on a screen per second in order that there may be no trace of flicker; the illumination of the brightest part of the brightest view, and that of the darkest part of the darkest view being

known; and a rough estimate from observations of the brightnesses of the pictures as usually projected by cinematographs on a screen would point to as many as 50 per second being necessary where the arc light is used for projection, though half this number would probably be sufficient to prevent the flicker from being distressing. This assumes that the photographs are "in register" on the screen.

It has also an important bearing on the usefulness of a "flicker photometer," for if I be the illumination to be measured, and n the number of revolutions per second when flicker vanishes, and k and k' constants, we have, as we have already seen,

$$n = k \log \mathrm{I} + k'.$$

Now the sensibility of the photometer depends upon the value of $dn/d\mathrm{I}$, which, from the above equation is equal to k/I, that is, it diminishes as I increases, thus the photometer is not adapted for the measurement of brilliant illuminations, at all events directly; and it must be remembered that, whether sudden or not, if there is a decided change in the value of k in the above expression in the relation between n and I at feeble illuminations, as fig. 2 indicates, the effect of this will be to increase the probable error of a flicker photometer, for k diminishes to practically half its former magnitude, hence the sensibility of such a photometer for feeble illuminations will be diminished by about one half.

Finally, if "l" be the "last" of the impression *undiminished*, measured in this experiment by the time the disc takes to make a half-revolution, $l = 1/2n$, and so l is given for the brighter illuminations considered by the equation

$$l = 1/(24 \cdot 8 \log \mathrm{I} + 20),$$

and for the fainter illuminations by

$$l = 1/(3 \cdot 12 \log \mathrm{I} + 53 \cdot 25),$$

the critical illumination being approximately that afforded by the sperm candle already mentioned at a distance of 2 metres.

Experiments were next made *to determine how the rate of rotation of a disc part white, part black, alters when the magnitude of the white sector is altered, the illumination of the disc by external sources being maintained constant for each set of experiments.*

In giving an account of these experiments and in discussing them it will be necessary to distinguish clearly between (1) the brightness of the white sector *at rest*, which is independent of its angular breadth, and depends solely on the distance of the disc from the source of illumination, and on the intrinsic illuminating power of the latter; and (2) the apparent brightness of the disc when rotating and *flickerless*, which depends *not only* on the external illuminating source (in fact,

on (1)), but also on the angular breadth of the white sector itself. If the words "in sense (1)" occur hereafter, they refer to the first of the above.

In the experiments which have just been described, it will be understood that since the disc remained throughout them half white, half black, its brightness when rotating was never disturbed by any alteration in the width of the white sector, and the results obtained connect "l," the "last" of the impression, with I, the brightness of the white sector at rest, or the degree of its illumination by the external source.

In what follows, the illumination afforded by the external illuminant is kept constant for each series of experiments, and the "last" is determined for the various brightnesses of the rotating disc caused by altering the angular magnitude of the white sector (and necessarily, of course, the angle of the black sector also).

The experimental details for the purpose are precisely the same as before described, save that separate black and white discs, dovetailed after Maxwell's method, were used instead of the single disc, half white half black. The white disc was gradated about the circumference in lengths subtending at the centre angles of 5°, and a concentric syren card mounted on the axis of the same motor, indicated by its note the speed of rotation.

The general character of the results may be, to some extent, foreseen from the results already obtained; if, as seems likely, the increase of stimulus given by increasing the width of the white sector is essentially of the same nature as that given by increasing the external illumination, though keeping the white sector constant, we should expect that the connection between n and the angular magnitude of the white sector w (measured here and elsewhere in degrees) would be of the form $n = f(\log w)$; and since $n = 0$ when $w = 0°$ or $w = 360°$, n is likely to be a function also of the product $w\,(360 - w)$. Experiment shows that for any constant illumination, using the word in sense (1), $n = a + b \log w\,(360 - w)$, where a and b are constants. b changes if the illumination (in sense (1)) changes. The results of the experiments are most clearly exhibited by the curves in fig. 3. Along the axis of X are plotted the notes given by the syren with the number of revolutions per second of the disc to which they correspond. The ordinates are proportional to the angular magnitude of the white sector, measured in degrees, from 0° to 360°. As it was proved that, within the errors of experiment, each curve was symmetrical about a line drawn parallel to the axis of X through the point corresponding to 180° on the axis of Y, only the lower halves of the curves given by experiment are drawn, and these are indicated in continuous lines. The true origin of these curves lies 55·73 of the paper-scale degrees to the left of the point B corresponding to 20 rotations of the disc per second. These five continuous lined curves give the relation ascertained experiment-

ally between n and the varying breadth of the white sector under five different illuminations (in sense (1)); these illuminations, being to one another as the numbers 1, 2, 4, 8, 16, consequently, by what has gone before, since the logs of these numbers are in A.P., the distances between points on these five curves, *corresponding to the same width of the white sector*, should lie, as they do, at equal distances measured parallel to the axis of X. The meaning to be attached to the fact that a point —say P in the figure on the curve marked 2—lies on this continuous lined curve is that, under an illumination called here "2" (actually that given by a 17·8 candle-power Ediswan lamp at a distance from the rotating disc of 3037 mm.), a disc of 110° white sector and 250° black must be rotated very approximately 32·3 times a second for flicker to vanish.

The dotted curves in the same figure are all derived from the general expression $n = a + b \log w\,(360 - w)$, by attributing to b values which depend on the logs of the different illuminations (in sense (1)) to which the disc was exposed in the five experiments by which the experimental curves were obtained. To make it clear how these curves are drawn, take the last curve on the right for example. The successive products of the numbers expressing the angular magnitudes of corresponding white and black sectors, *e.g.*, 10 and 350, 20 and 340, 30 and 330, &c., are calculated, and their logs taken; each logarithm is multiplied by a factor, b, which is constant for each curve, and which is itself of the form $c + d \log \mathrm{I}$, where c and d are constants, and I is the illumination in sense (1). The series of numbers thus obtained is multiplied by 100, and each number laid off in the paper-scale divisions as units from an origin which lies 132 paper-scale divisions to the left of the point B, corresponding to 20 rotations of the disc per second, and therefore lies 76·5 of the same divisions to the left of the origin of the continuous (experimental) curves, and these successive distances are the abscissæ of the points which have for ordinates lengths corresponding to the distances marked 10°, 20°, 30°, &c., on the axis of Y, being proportional to the angular magnitudes of the white sector in the successive products.

n stands for the number of rotations the disc makes per second when the flicker just vanishes, and it will be found that one division of the paper corresponds to 0·3588 of a rotation per second. Hence, if x is the abscissa of a point P on the experimental curve, reckoned in paper divisions, $n = x \times 0{\cdot}3588$ or $x = n/0{\cdot}3588$. If X is the abscissa, also in the divisions of the paper, for the same point P on the theoretical (dotted curves), we have

$$x = \mathrm{X} - 76{\cdot}5, \text{ and therefore } \mathrm{X} = x + 76{\cdot}5 = n/0{\cdot}3588 + 76{\cdot}5.$$

But $x = 100\, b \log w\,(360 - w)$, therefore the equation connecting n and w is $n/0{\cdot}3588 = -76{\cdot}5 + 100 \times b \times \log w\,(360 - w)$.

The value to be attributed to b must be of the form $c + d \log I$, where I is the illumination of the disc for the particular experiment in sense (1), because this has been already established by the results of the experiments expressed in fig. 3; and since the illuminations used to obtain the five curves were I, 2I, 4I, 8I, and 16I respectively, and I itself was the illumination of the disc by an incandescent lamp of measured brightness 17·8 (in terms of the standard candle before mentioned) at a distance of 3037 mm. from the disc, it will be found that I = 30·9 units (the unit being the illumination given by the same candle at a distance from the disc of 4000 mm.): $c + d \log I$ can therefore be written $c + d \log I + md \log 2$, where in the curves from I to XVI on the fig., m has the successive values 0, 1, 2, 3, and 4. Log I = 1·49 approx.; $c = 0{\cdot}2560085$; $d = 0{\cdot}0662837$, and the corresponding values of b become—

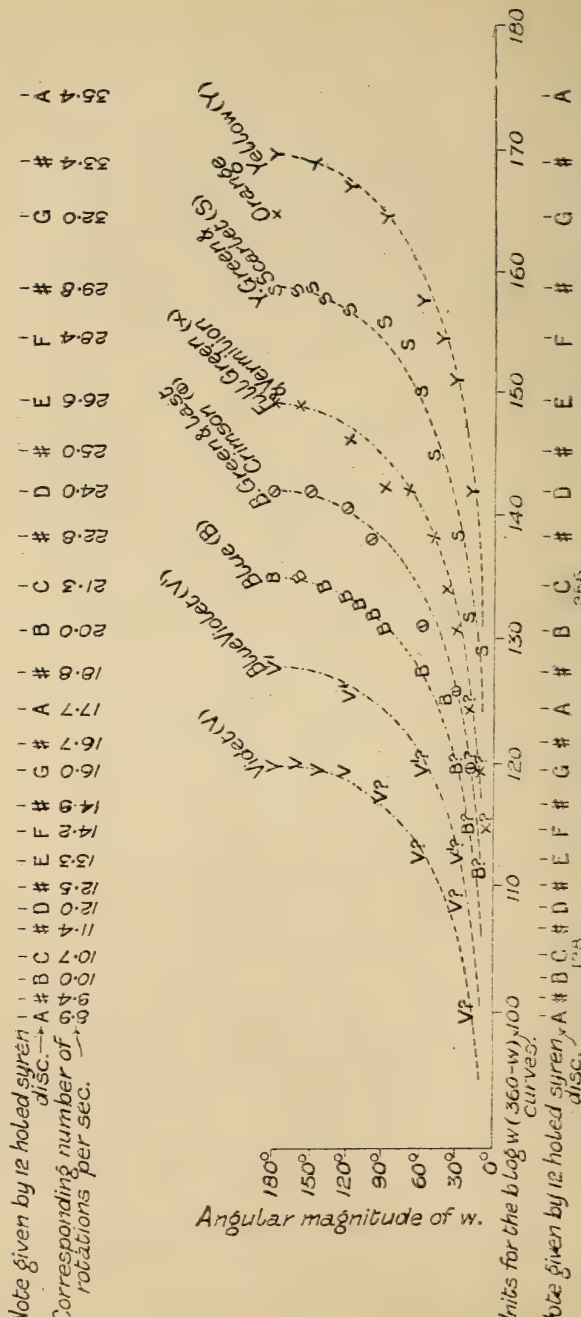
For the curve marked	I	0·3547722
,, ,,	II	0·3747256
,, ,,	IV	0·3946791
,, ,,	VIII	0·4146325
,, ,,	XVI	0·4345875

The equation to the curve I is $n/0{\cdot}3588 = -76{\cdot}5 + 100 \times 0{\cdot}3547722 \times \log w(360 - w)$, with similar equations for the other four curves.

Inspection of fig. 4 is sufficient to show how closely the theoretical and experimental curves coincide, but it may be well to take an example or two showing the same thing. We have seen that *experiment* shows that for flicker to vanish on a disc with a white sector of 110° under an illumination 2 × 30·9 units, it must be rotated very approximately 32·3 times per second (*vide* the point P, fig. 4). Now n can be calculated from the equation $n/0{\cdot}3588 = -76{\cdot}5 + 100 \times 0{\cdot}3747256 \times \log w(360 - w)$ by putting $w = 110$, when we find $n = 33{\cdot}23$. The *experimental* value of $n = 32{\cdot}3$ approx. Similarly the position of the point Q on the curve marked VIII shows that experiment proved that for a disc with a white sector of 130°, under an illumination I = 8 × 30·9 units, a rotation of approximately 39·4 times a second is necessary; and the result as found from the dotted curve whose equation is $n/0{\cdot}3588 = -76{\cdot}5 + 100 \times 0{\cdot}4146325 \times \log w(360 - w)$ by putting $w = 130$ will be found to be 39·13.

Experiments have proved that each of the experimental curves is symmetrical with respect to the horizontal line passing through the point on the axis of Y marked 180°, but only the lower half of the experimental curves has been drawn. (This is also true of fig. 4.) *The chief result* attained by these experiments is the knowledge of *the exact relation between the variation in the illumination of a disc, having a variable white sector, and the number of rotations per second necessary in order that*

FIG. 4.


Note given by 12 holed syren disc.
Corresponding number of rotations per sec.
Angular magnitude of w.
Units for the b log w (360-w) curves.
Note given by 12 holed syren disc.
Violet (V)
Blue Violet (V)
Blue (B)
B. Green & Last Crimson (Ø)
Full Green & Vermilion (X)
Y. Green & Scarlet (S)
Orange
Yellow (Y)

flicker may just vanish. This, so far as the writer is aware, has never been given before, and it seems remarkable that n should prove a *logarithmic* function of the illumination (whether the variation of illumination be due to the external source of light, or whether it be due to alteration in the magnitude of the white sector, the external illuminant remaining constant). It is also of interest to examine in the light of the results just mentioned the curves expressing the rate at which a disc of variable white sector must be rotated in the different colours of the same spectrum in order that flicker may just vanish. The figure 3* gives these curves, as found under the conditions there described. Since the publication of that paper, the experiments there described have been repeated, with the additional precautions mentioned in the present paper, with the result that whilst the general form of the curves has been confirmed, the central part of each, represented in fig. 3 as a vertical straight line, has been proved to be slightly curved, with the concave side facing the axis of Y. These redetermined curves are shown in fig. 4, for the principal colours of a spectrum (obtained by means precisely similar to those used in the construction of the curves in the first paper, and which need scarcely be repeated here), the dotted curves on the same figure being those derived from $x = b \log w (360 - w)$, and $y = w$, where b is a constant for each curve, and w is the angular magnitude of the white sector, measured in degrees. The seven constants for these curves are—

For the violet	$0 \cdot 2660459 \times 100$
,, blue-violet	$0 \cdot 2837823 \times 100$
,, blue	$0 \cdot 2993016 \times 100$
,, blue-green and crimson	$0 \cdot 3148209 \times 100$
,, full green and vermilion	$0 \cdot 3303403 \times 100$
,, yellow-green and scarlet	$0 \cdot 3547722 \times 100$
,, yellow	$0 \cdot 3747256 \times 100$

It will at once be evident that the curves for the different colours coincide, within the errors of experiment, with the dotted curves, and this fact proves that the duration of the impression of the different spectral colours, undiminished, depends *solely* on the luminosity of the colours, and not on their wave frequency. This has been suspected before,† but it has never, so far as the writer knows, been definitely proved. The proof here given is, that, so far as flicker phenomena go, we obtain exactly the same curves, whether we place the disc in the different colours of the spectrum, or subject it to white light, and

* 'Roy. Soc. Proc.,' vol. 63, p. 352.

† *Vide* a paper by Professor E. L. Nichols, 'American Journal of Science,' 1884, No. 28, pp. 243 to 252.

vary the intensity of its illumination by the white light. We can, therefore, by the help of what has already been said, deduce from the above constants for each curve in fig. 4, the illumination of the disc at each point of the spectrum considered, for it follows from the nature of the curves in fig. 3, that each of the above seven constants is of the form $c + d \log I$, where $c = 0{\cdot}2560085$, and $d = 0{\cdot}066283$, and I is the measure of the illumination to which the disc is exposed in terms of the unit illumination already used. We have, therefore,

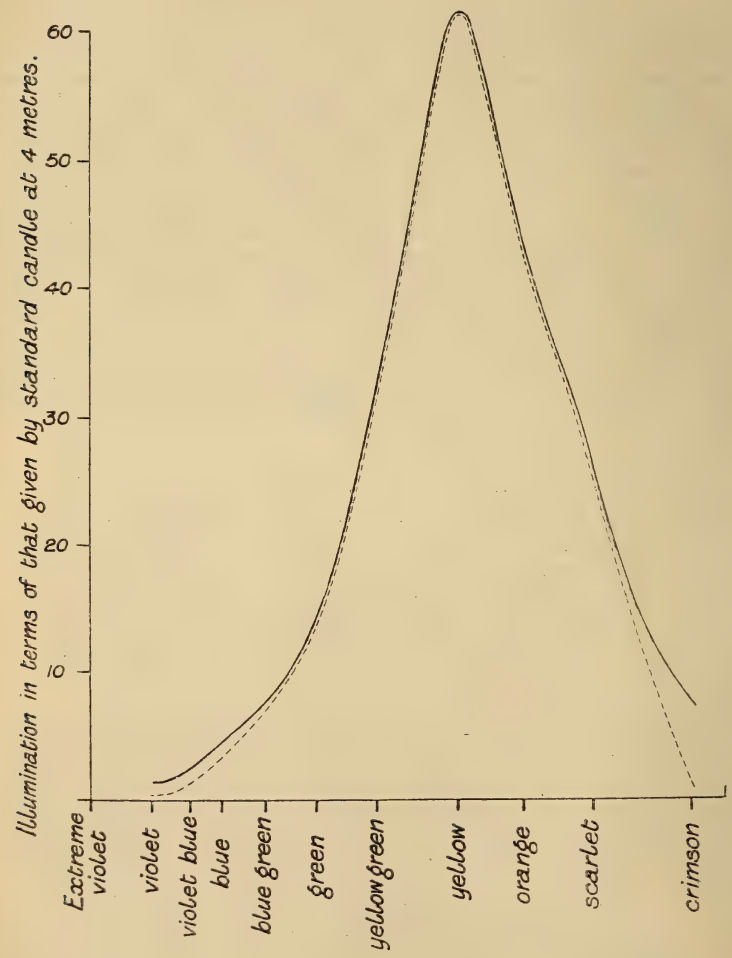


Fig. 5.

for the violet curve $0{\cdot}2660459 = 0{\cdot}2560085 + 0{\cdot}066283 \times \log I$ whence $I = 1{\cdot}42$ approx. Similarly for the other colours in the order of the curves from left to right, we find $I = 2{\cdot}62, 4{\cdot}50, 7{\cdot}71, 13{\cdot}22,$

30·9, 61·8: these values are expressed in the curve of fig. 5, which is therefore a curve expressing roughly the luminosity of different parts of the same spectrum according to the evidence afforded by flicker. For the sake of comparison the luminosity of the different colours as measured by Vierordt is indicated by a dotted curve, and it will be seen that the two curves practically coincide, except for the extreme parts of the spectrum; it should, however, be stated that Vierordt's curve has been drawn by making his value for the luminosity of the yellow coincide with that given for this colour by the continuous curve in the figure, and reducing his other values in the same proportion: the reduced values are then placed within the limits of the colours to which they are assigned by Vierordt. Moreover, Vierordt's curve is for a solar prismatic spectrum, whilst the other is for incandescent lime, used with a grating.* Lastly it should be remembered that the disc was very feebly illuminated in the violet and extreme crimson, and also, for the small values of the white coloured sector, in the blue, blue-green, and vermilion, so that the alteration of the relation between n and log I (indicated in fig. 2 by the steeper line for feeble illuminations) should probably be considered: if this is so, the two curves will be brought into still closer union. The writer postpones the consideration of this matter, until he has had the opportunity of trying further experiments with such feeble illuminations.

"The Refractive Indices of Fluorite, Quartz, and Calcite." By J. WILLIAM GIFFORD. Communicated by SILVANUS P. THOMPSON, F.R.S. Received February 5,—Read February 13,—Received in revised form May 7, 1902.

1. *Method of Observation.*—Measurements of fluorite, quartz, and calcite have been made by Rudberg, Mascart, Cornu, Sarasin, Glazebrook, Van der Willigen, Vogel, Pulfrich, Rubens, Baille, and many others. Those now offered were originally undertaken with the view of further extending the range and accuracy of lenses constructed of these substances. A new method of obtaining the refractive indices has been adopted. Each of the angles of the prisms used was as nearly as possible 60°. When this is the case it is sufficient to measure the deviation of light of a definite wave-length at each angle in turn; the mean of these deviations may be taken as the deviation corresponding

* C. Vierordt, 'Pogg. Ann.,' vol. 137, p. 200.

to an angle of 60°, and if D denote this mean, the refractive index $\bar{\mu}$ is given very approximately by the formula*

$$\bar{\mu} = \sin \tfrac{1}{2}(D + 60^\circ)/\sin 30^\circ.$$

The method has several advantages. In the first place it is not necessary to measure the angles of the prism with accuracy; then, again, if the prism be suitably placed, light reflected from the outside of the base enters the telescope and is only parallel to the rays of the wave-length under measurement if those rays have passed through the prism parallel to its base. Thus we have two images of the slit, one by refraction the other by reflection, in the field of view in the same direction when the condition for minimum deviation is satisfied. When they overlap in the field of view of the eye-piece, we may, therefore, rest assured that we have minimum deviation for the wave-length under observation. By sliding the prism in a direction at right angles to its base, it is easy to regulate the amount of light thus reflected from the outer side of the base.

2. *Instruments.*—A special spectrometer by Hilger with objectives of quartz, the collimator provided with bars carrying the spark apparatus

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* The following investigation by Dr. Glazebrook will show the amount of error introduced by the method:—

Let D be the deviation and A the angle of the prism; and let $D + \delta$ be the deviation corresponding to the angle $A + \alpha$. Then we have

$$\frac{\sin \dfrac{D + A}{2}}{\sin \dfrac{A}{2}} = \mu = \frac{\sin \dfrac{D + \delta + A + \alpha}{2}}{\sin \dfrac{A + \alpha}{2}},$$

whence we find, if α be small,

$$\delta = \alpha \cdot \frac{\sin \dfrac{D}{2}}{\sin \dfrac{A}{2} \cos \dfrac{D + A}{2}} + \frac{\alpha^2}{4} \cdot \frac{\sin \dfrac{D + 2A}{2} \sin \dfrac{D}{2}}{\sin^2 \dfrac{A}{2} \cos^2 \dfrac{D + A}{2}} \tan \frac{D + A}{2}.$$

Now, if α_1, α_2, α_3 be the differences from 60° of the three angles of one of the prisms, each measured in circular measure, D, the deviation which would be observed if the angle were 60°, and $D + \delta_1$, $D + \delta_2$, $D + \delta_3$, the actually observed deviations, then A is 60° and $\alpha_1 + \alpha_2 + \alpha_3$ is zero. Hence, if $\bar{\mu}$ be the refractive index as found by the method adopted in the paper, and μ the true refractive index, we have

$$\mu = \frac{\sin \frac{1}{2}(D + A)}{\sin \frac{1}{2} A}, \quad \bar{\mu} = \frac{\sin \frac{1}{2}\{D + A + \frac{1}{3}(\delta_1 + \delta_2 + \delta_3)\}}{\sin \frac{1}{2} A};$$

and a condensing lens also of quartz, the whole of more than ordinarily solid construction, was used for the work. On this instrument each degree of the circle is subdivided into 12 parts (of 5′ each), and the wheel of the micrometer, one revolution of which corresponds with one subdivision of the circle, is divided into 5 parts (of 1′ each), and subdivided into 300 parts (of 1″ each), thus rendering the measurement of seconds of arc, and with care fractions of seconds, possible. Readings were always taken to 0·25″, the quartz fibre of the micrometer being brought into position by turning the screw in one direction only. In dividing the circle, burrs are thrown up by the engraving tool on each side of the cut, and reflections from either or both of these appear as fine white lines in the reading microscope. The quartz fibre is made to cover each of these reflections in turn, and the mean taken. Further details of the method are given in the list of indices.

3. *Temperature.*—In order to reduce errors due to change of temperature to a minimum, I have kept a standard thermometer on the prism table, and have never attempted to regulate the temperature by raising or lowering that of the room, without allowing at least 2 hours to elapse before making observations, so as to ensure the prism attaining the same temperature. In most cases times were chosen for observation when the temperature was, without artificial means, found to be that required. The temperature adopted for observation was 15° C. In the very few cases in which observations took place at

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and, substituting for δ, &c., this gives

$$\bar{\mu} = \mu \left\{ 1 + \tfrac{1}{24}(\alpha_1^2 + \alpha_2^2 + \alpha_3^2) \frac{\sin\frac{D}{2}\sin\frac{D+2A}{2}}{\sin^2\frac{A}{2}\cos^2\frac{D+A}{2}} \right\}.$$

Putting in the small term D = 40° and A = 60°, the value of the trigonometrical expression is found to be 3 approximately.

Hence,

$$\bar{\mu} = \mu \{1 + \tfrac{1}{8}(\alpha_1^2 + \alpha_2^2 + \alpha_3^2)\}.$$

If α, &c., be in minutes, since the circular measure of a minute is 1/3420, or 0·00029,

$$\bar{\mu} = \mu \{1 + 0{\cdot}00000001\,(\alpha_1^2 + \alpha_2^2 + \alpha_3^2)\}.$$

For fluorite, $\alpha_1 = 1\frac{1}{5}$, $\alpha_2 = -2\frac{1}{4}$, $\alpha_3 = \frac{1}{2}$, so that

$$\bar{\mu} = \mu \{1 + 0{\cdot}00000007\}.$$

The error is almost unity in the seventh figure, which is quite negligible. For the other prisms it is less.

higher temperatures, recourse was had to the table of temperature refraction coefficients. These coefficients are the mean of a separate series of measurements carried out at temperatures at least 10° C. apart.

4. *Material.*—The material of which the prisms were made is quite normal. The fluorite is from Germany, the quartz from Brazil, the calcite from Iceland.

5. *Standard Wave-lengths.* — Rowland's wave-lengths have been adopted wherever available. As a specimen of the work in fullest detail the determination of the index of line C for fluorite is given. (See Appendix I.)

6. *Curves.*—A very severe method of testing refractive indices, when they are fairly close together, is to calculate the form of a thin lens from two of the substances, so as to be achromatic for two definite wave-lengths, and then focal lengths calculated by the formula

$$(\mu - 1)\left(\frac{1}{r} - \frac{1}{s}\right) = \frac{1}{F}$$

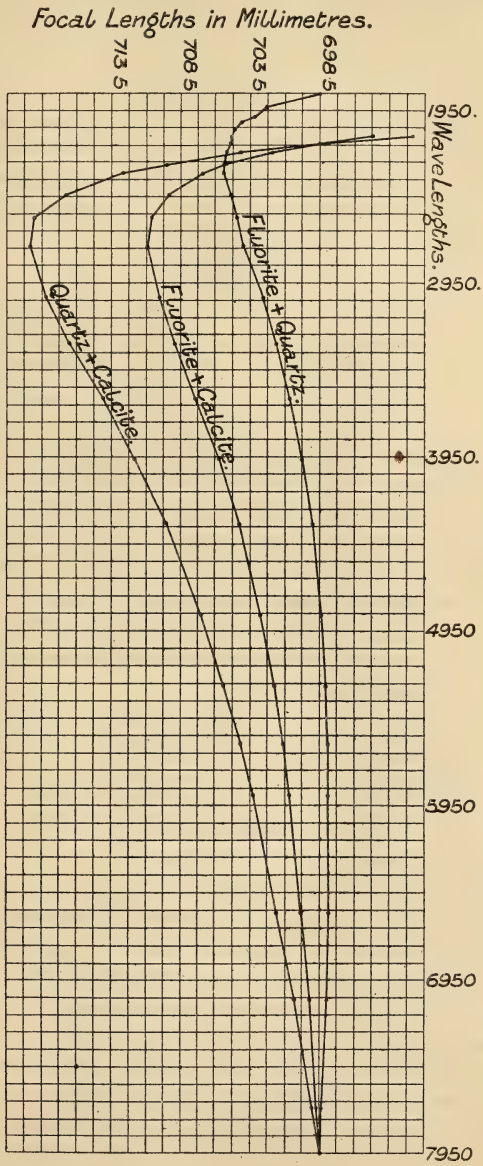
for the different wave-lengths should form the ordinates of a smooth curve. The focal length 6985 mm. was taken as being ten times that required for the lenses of a large spectrometer now just completed. The curves shown (p. 333) are for (A) fluorite and quartz, (B) fluorite and calcite, (C) quartz and calcite. It appears that the focal length of the fluorite and quartz combination is very nearly independent of the wave-length. A list of such focal lengths is given in Appendix II.

7. *Measure of Error.*—An approximate estimate of the error in the deviations may be made as follows :—In reducing a set of observations three means, called "group deviations" (see Appendix I), are taken. Let x, y be values of the two of these which show the greatest difference, then the quantity $a = \frac{x-y}{3}$ may be taken as a measure of the error. There are 118 measurements in the table for

33 of which	a = less than	$\frac{2}{3}''$;	corresponding variation of index			= 0·0000023
39	„ =	„ $1\frac{1}{3}''$;	„	„	„	= 0·0000034
31	„ =	„ $2\frac{2}{3}''$;	„	„	„	= 0·0000084
15	„	= more than $2\frac{2}{3}''$;	„	„	„	= „
1 only where	a = as much as	$5\frac{1}{2}''$;	„	„	„	= 0·0000150

Optical Correctness of Prisms.—In order to test whether the faces of the quartz and calcite prisms were parallel to the optic axis, each of the angles of the prisms was measured, and from these measurements and the deviations proper to them separate indices were calculated for each of the three angles (*a*) for the ordinary ray, (*b*) for the extraordinary ray. Owing for the most part to errors inseparable from measurements

Chromatic Curves for thin Doublets of approximately 7000 millimetres focus.



of angle, the indices are not quite the same at each angle, but if the variations are in the same direction and not much greater for the extraordinary than for the ordinary ray, we may assume that the optic axis is very nearly perpendicular to the principal plane. Taking the three angles as α, β, and γ, the results are as follow (for line D):—

Quartz.

Ordinary ray.		Extraordinary ray.	
α.	1·5442550	α.	1·5533605
β.	1·5442306	β.	1·5533356
γ.	1·5442765	γ.	1·5533977

Calcite.

Ordinary ray.		Extraordinary ray.	
α.	1·6583705	α.	1 4864145
β.	1·6583381	β.	1·4863865
γ.	1·6583595	γ.	1·4864062

Thus it would seem that the differences in values of the extraordinary indices are not greater than those for the ordinary ray.

As further evidence of accuracy in observation, it may be mentioned that the measurements for line D quartz, ordinary and extraordinary ray recorded in the table, were made on March 4, 1900. On December 27, 1901, the prisms having been set up by mistake for a prism of left-handed quartz, a complete independent series of measurements were again made for both rays before the mistake was discovered. The two resulting indices follow:—

	Ordinary ray.	Extraordinary ray.
March 4, 1900.....	1·5442558	1·5533662
December 27, 1901.....	1·5442558	1·5533673

The difference of the two measurements of the ordinary ray begins in the 8th decimal place and is not shown.

8. *Probable Accuracy of Indices.*—These refractive indices may be considered correct to the 5th decimal place; the 6th is only approximately correct, and the 7th is of little value. But it is believed that in many cases the error does not exceed unity in the 6th place.

9. *Interpolation.*—The question of the relation between refractive index and wave-length has not been gone into, but it may be said that for the ultra-violet Cauchy's formula is not of great value, while for the visual spectrum Nelson's formula is a nearer approximation, and takes little time to work out,* while the graphical interpolation of deviations by squares of reciprocals proposed by Dr. Marshall Watts† is not

* See 'R. M. S. Journal,' April, 1899, Presidential Address.

† 'Index of Spectra,' p. xii.

very inferior to Nelson's, as the indices can soon be calculated, and it can easily be turned into simple arithmetic without the drawing. But the indices here given have all been observed. There has been no attempt to make them fall into line by any kind of correction, such as the use of a freehand curve or by interpolation.

10. *Right and Left Quartz.*—Some doubt having arisen as to the indices of right-handed quartz being the same as those of left-handed, I have had prepared by Mr. Hilger a prism of left-handed quartz of the same dimensions as the prism of right-handed quartz used in the foregoing measurements. I have with this prism determined the indices, ordinary and extraordinary, for the mean D line, and have repeated the corresponding observation with the right-handed prism used in the previous determinations, as before referred to. The results are as follows:—

	Ordinary.	Extraordinary.
March 4, 1900. Right quartz.....	1·5442558	1·5533662
December 27, 1901. Right quartz...	1·5442558	1·5533673
January 9, 1902. Left quartz	1·5442363	1·5533452
Difference	0·0000195	0·0000221

The value given for left quartz and the ordinary ray by Van der Willigen (for wave-length 5895·37, which he names D) is 1·54417.

Finally a rough determination of the specific gravities was made as follows:—

Right quartz.	Left quartz.
2·6495	2·6511

It will thus be seen that while the refractive indices of the right-handed quartz are greater than those of the left-handed, the specific gravity is less.

11. *Results of the Measurements.*—The results of the observations are given in the following table of refractive indices:—

Table of the Refractive Indices of Fluorite, Quartz, and Calcite at 15° C.

Wave-length.	Fluorite.	R. quartz, ordinary.	R. quartz, extraordinary.	Calcite, ordinary.	Calcite, extraordinary.	
7950 Rb	1·4306394	‡1·5385126	‡1·5474212	1·6488640	1·4821645	* Wave-lengths for mean intensities of double lines or groups.
*7682·45 K_a(A′)	1·4309494	1·5390604	1·5479969	1·6497403	1·4825523	
*7065·59 He(B′)	1·4317107	1·5405000	1·5494865	1·6520702	1·4835315	
6563·04 H_a(C)	1·4325233	1·5419306	1·5509480	1·6543988	1·4845685	
*5893·17 Na(D)	1·4338542	1·5442558	1·5533662	1·6583551	1·4863915	
Maximum 5607·1 Pb(A) visual effect	1·4345651	1·5454751	1·5546189	1·6604550	1·4873449	† As far as observations go, the coefficients for other wave-lengths are in the ratio of their refractive indices.
*5270·11 Fe(E)	1·4355641	1·5471766	1·5563881	1·6634145	1·4887055	
4861·49 Hβ(F)	1·4370666	1·5497003	1·5589883	1·6678308	1·4907407	
4340·66 Hγ(G′)	1·4396260	1·5539758	1·5634058	1·6755174	1·4942419	
3961·68 Al	1·4421883	1·5582352	1·5678355	1·6832956	1·4977742	
Ultra-violet.						
3610·66 Cd	1·4453386	1·5634731	1·5732431	1·6931606	1·5022352	‡ Right-handed quartz; for left-handed quartz, see paper.
*3302·85 Zn	1·4490705	1·5697424	1·5797292	1·7051544	1·5074581	
3034·21 Sn	1·4533819	1·5769921	1·5872306	1·7195881	1·5136464	
2748·68 Cd	1·4596612	1·5875286	1·5981316	1·7415041	1·5226617	
2573·12 „	1·4647726	1·5962464	1·6071638	1·7605000	1·5301212	
*2445·86 Ag	1·4696500	1·6046194	1·6158596	1·7796645	1·5373101	
2312·95 Cd	1·4751617	1·6140337	1·6256465	1·8023942	1·5455001	

2265·13 Cd	1·4775433	1·6181975	1·6299653	1·8130351	1·5491444
2194·4 „	1·4814525	1·6249914	1·6370224	1·8307980	1·5551219
2144·45 „	1·4845693	1·6304659	1·6427036	1·8458240	1·5599225
2098·8 Zn	1·4875705	1·6356982	1·6481440	1·8608110	
*2062·0 „	1·4902594	1·6403981	1·6530369	*Note.*—After W.L. 2144·45 Calcite rapidly loses transparency. W.L. 2098·8 was measured with a special thin prism, cut perpendicularly to the axis. With the prisms polished on three faces the axis was parallel to the edge.	
2024·2 „	1·4931822	1·6455837	1·6584434		
1988·1 Al	1·4961305	1·6509174	1·6639872		
1933·5 „	1·5012262	1·6600266	1·6734876		
1852·2 „	1·5098894	1·6758953	1·6900687		
Temperature Refraction Coefficients of Wave-length 5893·17 (D)† for 1° C.					
	−0·00001022	−0·00000519	−0·00000635	0·00000479	0·00001447

The refractive indices of Fluorite and Quartz decrease with rise of temperature, those of Calcite increase.

Method of Observation.

In cutting the goniometric circle a burr is thrown up by the engraving tool on each side of every division. By two small electric lamps suitably arranged behind the reading microscope, either or both burrs are made to appear as fine white lines. With the help of quartz fibres, measurements are made on these, and the mean taken. For every index measurements were made thus: on either side of each of the three angles of the prism, and at three positions of the circle (zero = 0°, 120°, and 240°), thirty-six measurements in all, except in one case (Calcite ordinary ray, wave-length 2098·8), for which a special thin prism which could be polished on two sides only was necessary, and measurements at one angle only could be made. The rays from the collimator were made parallel for each wave-length. These refractive indices may be considered correct to the fifth decimal place, the sixth is only approximately correct, the seventh of little value.

Appendix I.

Details of an Observation as a Specimen.

October 28, 1900. Zeiss Fluor Prism, 3 sides. Temp. 59° F.
W.L. 6563·0 C. (H_α).

	Left of prism.					Right of prism.	
			31	30	36·25		
	31	31	0·5 ×	left burr	328	29	45·25
α (0°)		30	35·25	mean		29	22·75
		30	21·25 ×	right burr		29	9·25
			31	28	5·125		
	151	28	25·0		88	32	11·25
β (120°)		28	0·0			31	49·75
		27	41·75			31	31·75
			31	30	4·0		
	271	30	16·0 ×		208	30	11·0 ×
γ (240°)		29	54·75			29	46·75
		29	43·5			29	32·0
			A. 31	29	35·125		
			31	28	8·75		
	31	28	39·0		328	32	21·75
β (0°)		28	12·25			31	54·75
		27	52·25			31	31·5
			31	29	56·625		
	151	20	7·5		88	30	19·5
γ (120°)		29	47·0			29	53·75
		29	36·25			29	37·75
			31	30	43·0		
	271	30	58·5 ×		208	29	34·0
α (240°)		30	36·25			29	10·25
		30	25·25 ×			28	55·0
			B. 31	29	36·125		
			31	29	59·0		
	31	30	17·0 ×		328	30	19·25
γ (0°)		29	53·25			29	55·25
		29	39·5			29	41·25
			31	30	37·875		
	151	30	51·5 ×		88	29	40·5
α (120°)		30	30·25			29	14·5
		30	20·0 ×			28	57·25
			31	28	15·25		

	271	28	39·0		208	32	6·5
β (240°)		28	13·75			31	43·25
		27	55·25			31	23·5
			C. 31	29	37·375		

Result.

Dev. = 31° 29′ 36″·208. Ref. Index = 1·4325233.

The above is an exact copy of the original observation for line C (hydrogen α). It will be seen that the readings of the micrometer are arranged in three groups, each of which is complete in itself, since it contains readings at all the angles and at each of the three positions of the circle. The averages for each group, called A, B, and C, are underlined twice, and the final result is the mean of these three averages. The three angles of the prism are called α, β and γ: each of these gives the deviation under measurement three times, that is, once at each of the three positions of the circle (zero = 0°, 120° and 240°). The two burrs thrown up by the engraving tool on each side of every division, already described, give the first readings, and in taking the mean of these a deduction of 1″ for every 1′, measured in the microscope, is made for the optical error of the reading microscope, a careful measurement of the error having come out within a small fraction of this. The microscope was furnished with a fixed and a movable quartz fibre; sometimes the movable fibre was too close to the other to read with accuracy, and then had to be moved off to the next division of the circle (5′ further off centre). In this case I have marked the reading with a ×, so as to ensure attention being called to the matter. After turning the same angle of the prism round to the other side as usual, another mean is obtained from two burr readings in the same way. These two means, one on each side, underlined once, give by the usual method the value for each angle in the groups already described.

Appendix II.

Table giving the Focal Lengths in metres of a Compound Lens of Fluorite and Quartz achromatised for Wave-lengths 7950 and 1852.

Radii.—R = 0·28358015, S = − 0·20801615, R′ = S, S′ = ∞, R, S, R′, S′ refer to the surfaces of the two lenses.

Wave-length.		Focal length.	Wave-length.	Focal length.
7950		1·00000	2749	0·99382
7652	A′	1·00005	2573	0·99314
7065	B′	1·00063	2445	0·99275
6563	C	1·00073	2313	0·99208
5893	D	1·00081	2265	0·99225
5607	A	1·00076	2194	0·99233
5270	E	1·00061	2144	0·99267
4861	F	1·00022	2099	0·99281
4341	G′	0·99945	2062	0·99299
3962		0·99857	2024	0·99356
3611		0·99750	1988	0·99461
3303		0·99654	1933	0·99563
3034		0·99548	1852	1·00000

Graphical representations are given in the curves on p. 333, the achromatised focal length being there 6985 instead of 1000 mm.

Appendix III.

Partial and Proportional Dispersions of Fluorite, Quartz, and Calcite, and their Combinations.

Substance.	C to F ($\delta\mu$).	$\mu_D - 1$.	$\frac{\mu - 1}{\delta\mu} = \nu$.	$\frac{\nu}{\nu'} = N$.	A′ to D.	D to F.	F to G.
Fluorite	0·0045433	0·4338542	97·493		0·0029048	0·0032124	0·0025594
					0·63936	0·70706	0·56333
				1·392	0·02931	−0·00633	−0·01306
Quartz	0·0077697	0·5442558	70·048	..	0·0051954	0·0054445	0·0042755
					0·66867	0·700735	0·55027
				1·438	−0·02731	0·00473	0·02199
Calcite	0·0134320	0·6583551	49·013	..	0·0086148	0·0094757	0·0076866
					0·64136	0·70546	0·57226
				1·989	0·00200	−0·00160	0·00893
Fluorite.....	0·0045433	0·4338542	97·493	..	0·0029048	0·0032124	0·0025594
					0·63936	0·70706	0·56333

Note.—The last three columns give details as to the dispersions between the lines indicated. In each group of three lines connected with a given material the first line gives the differences in refractive indices, the second line the ratio of these differences to the mean dispersion $\delta\mu$, and the third line, in old-faced type, the outstanding secondary dispersion when lenses of the given materials are combined.

"Note on a Magnetic Detector of Electric Waves, which can be employed as a Receiver for Space Telegraphy." By G. MARCONI, M.I.E.E. Communicated by Dr. J. A. FLEMING, F.R.S. Received June 10,—Read June 12, 1902.

The present note bears upon the special manner in which a core or rod of iron or steel placed in a varying magnetic field is affected by high-frequency oscillations transmitted from considerable distances.

The magnetisation and demagnetisation of steel needles by the effect of electrical oscillations has long been known, and has been noted especially by Professor J. Henry, Abria, Lord Rayleigh, and others. Mr. E. Rutherford also has described a magnetic detector of electric waves, based on the partial demagnetisation of a small core composed of fine steel needles, previously magnetised to saturation, and placed in a solenoid of fine copper wire connected to exposed plates. By means of a magnetometer Mr. Rutherford succeeded in tracing the effects of his electrical radiator up to a distance of $\frac{3}{4}$ mile across Cambridge.*

The detector which I am about to describe is, in my opinion, based upon the decrease of magnetic hysteresis which takes place in iron when, under certain conditions, it is exposed to the effect of high-frequency or Hertzian waves.

As employed by me up to the present, it has been constructed in the following manner:—On a core or rod consisting of thin iron wires are wound one or two layers of thin insulated copper wire. Over this winding, insulating material is placed, and over this again, another longer winding of thin copper wire contained in a narrow bobbin.

The ends of the winding nearest the iron core are connected to the plates or wires of the resonator, or as is the usual practice in long-distance space telegraphy, to earth and to an elevated conductor; or they may be connected to the secondary of a suitable receiving transformer or intensifying coil, such as are now employed for syntonic wireless telegraphy. The ends of the other winding are connected to the terminals of a telephone or other suitable receiving instrument. Near the ends of the core, or in close proximity to it, is placed a magnet, preferably a horse-shoe magnet, which, by a clockwork arrangement, is so moved or revolved as to cause a slow and constant change, or successive reversals, in the magnetisation of the iron core. I have noticed that if electrical oscillations of suitable period be sent from a transmitter according to the now well-known methods, rapid changes are effected in the magnetisation of the iron wires, and these

* See 'Phil. Trans.,' A, vol. 189 (1897), pp. 1—24.

changes necessarily cause induced currents in the windings, which induced currents in their turn reproduce on the telephone with great clearness and distinctness the telegraphic signals which may be sent from the transmitting station.

Should the magnet be taken away, or its movement stopped, the receiver ceases to be perceptibly affected by the electric waves, even when these are generated at very short distances from the radiator.

This detector has been successfully employed for some time in the reception of wireless telegraphic messages between St. Catherine's Point, Isle of Wight, and the North Haven, Poole, over a distance of 30 miles, and also between Poldhu, in Cornwall, and the North Haven, over a distance of 152 miles, of which 109 are over sea and 43 over high land. It has also been ascertained that signals can be obtained over these distances with the new detector when employing less power at the transmitting station than is necessary if a reliable coherer be substituted for the magnetic detector. I have had occasion to notice, however, that the signals audible in the telephone are weakest when the poles of the rotating magnet have just passed the core and are increasing their distance from it, whilst they are strongest when the magnet poles are approaching the core.

Very good results have also been obtained by keeping the magnet fixed, and using an endless iron rope or core of thin wires revolving on pulleys (worked by a clockwork arrangement), which cause it to travel through the copper wire windings, in proximity to a horse-shoe magnet, or, preferably, two horse-shoe magnets with their poles close to the windings, and with their poles of the same sign adjacent. In this case the copper wire windings are separated from the iron by means of a stiff, thin pipe of insulating material in order to prevent chafing of the wires. With this arrangement the signals appear to be quite uniform in strength.

There appears to be a certain magnetic force which gives best results, but different qualities of iron require different values. There would also appear to be a particular speed of revolution for the magnets employed which is more suitable than any other. I have obtained good results when causing the magnets to revolve at the rate of one revolution every 2 seconds, or, when using a moving core, by causing it to travel at a speed of about 30 cm. in 4 seconds.

Either iron or steel can be used for the cores or revolving rope, but I have observed that by far the best effects are obtained when using hard-drawn iron wires or iron wire that has been considerably stretched or twisted beyond its limits of elasticity prior to its employment.

I have used cores generally consisting of about thirty hard-drawn iron wires of approximately 0·5 mm. in diameter, with a winding on them made up of a single layer of silk-covered copper wire 0·019 cm. in diameter and of a total length of 2·4 metres. The other winding, con-

nected to the telephone, has consisted of similar wire, and I have been in the habit of employing a sufficient number of turns of it to give a resistance about equal to that of the telephone used.

It would, no doubt, be possible to obtain the signals by causing the iron core to act directly on a telephone diaphragm, and in this case the secondary winding on the core could be omitted. The length of the electric waves used in the experiments between St. Catherine's Point and North Haven was about 200 metres. If longer waves are employed, it is desirable that the length of the winding nearest the iron should be increased.

This detector, as I have already stated, appears to be more sensitive and reliable than a coherer, nor does it require any of the adjustments or precautions which are necessary for the good working of the latter.

Further advantages in its use become apparent when it is employed in connection with my syntonic system of space telegraphy. According to this system, electrical syntony between the transmitter and receiver is dependent on the proper electrical resonance of the various circuits of transformers used in the receivers. With certain coherers one difficulty has been that it was not always possible to restore them by mechanical tapping to the same electrical resistance which they possessed before being affected by the transmitted electric waves, the result being that the secondaries of the receiving transformers were at certain times open and at other times closed by a variable resistance, thus causing an appreciable variation in their natural period of electrical oscillation.

The magnetic detector which I have described possesses, on the other hand, a practically uniform and constant resistance much lower than that of a coherer in its sensitive condition, and, as it will work with a much lower E.M.F., the secondaries of the tuning transformers can be made to possess much less inductance, their period of oscillation being regulated by a condenser in circuit with them, which condenser may be much larger (in consequence of the smaller inductance of the circuit) than those used for the same period of oscillation in a coherer circuit, with the result that the receiving circuits can be tuned much more accurately to a particular radiator of fairly persistent electric waves.

The considerations which led me to the construction of the above-described detector are the following:—It is a well-known fact that after any change has taken place in the magnetic force acting on a piece of iron, some time elapses before the corresponding change in the magnetic state of the iron is complete. If the applied magnetic force be either subjected to a gradual increase followed by an equally gradual diminution, or caused to effect a cyclic variation, the corresponding induced magnetic variation in the iron will lag behind the changes in the applied force. To this tendency to lag behind, Professor Ewing has given the name of Magnetic Hysteresis.

It has been shown also by Gerosa, Finzi, and others that the effect of alternating currents or high-frequency electrical oscillations acting upon iron is to reduce considerably the effects of magnetic hysteresis, causing the metal to respond much more readily to any influence which tends to alter its magnetic condition. The effect of electrical oscillations probably is to bring about a momentary release of the molecules of iron from the constraint (or viscosity) in which they are ordinarily held, diminishing their retentiveness, and consequently decreasing the lag in the magnetic variation taking place in the iron.

I therefore anticipated that the group of electrical waves emitted by each spark of a Hertzian radiator would, if caused to act upon a piece of iron which is being subjected at the same time to a slowly varying magnetic force, produce sudden variations in its magnetic hysteresis, which variations would produce others of a sudden or jerky nature in its magnetic condition. In other words, the magnetisation of the iron, instead of slowly following the variations of the magnetic force applied, would at each spark of the transmitter suddenly diminish its magnetic lag caused by hysteresis.

These jerks in the magnetic condition of the iron would, I thought, cause induced currents in a coil of wire of strength sufficient to allow the signals transmitted to be detected intelligibly on a telephone, or perhaps even read on a galvanometer.

The tests to which I have referred above confirm my belief that the magnetic detector can be substituted for the coherer for the purposes of long-distance space telegraphy.

"A Note on the Effect of Daylight upon the Propagation of Electromagnetic Impulses over Long Distances." By G. Marconi, M.I.E.E. Communicated by Dr. J. A. Fleming, F.R.S. Received June 10,—Read June 12, 1902.

During some long-distance space telegraphy tests carried out towards the end of February last between a transmitting station situated at Poldhu, on the coast of Cornwall, and a receiving station on board the U.S. s.s. "Philadelphia" travelling from Southampton to New York, I had the opportunity of noticing for the first time in my experience, considerable differences in the distances at which it was possible to detect the received oscillations during daylight, as compared with the distances at which the effects could be obtained at night.

Before describing the results obtained, it may be useful if I give a

brief description of the nature of the apparatus used at the transmitting and receiving stations.

The transmitter at Poldhu was similar in principle to that used by me in previous work,* but the elevated conductor at the transmitting station was much larger, and the potential to which it was charged at the peak of each electrical oscillation very much in excess of any that had been previously employed. The transmitting elevated conductor consisted of fifty almost vertical naked copper wires, suspended at the top by a horizontal wire stretched between two poles each 48 metres high and placed 60 metres apart.

These wires were separated from each other by a space of about 1 metre at the top, and, after converging together, were all connected to the transmitting instruments at the bottom. The potential to which these conductors were charged during transmission was sufficient to cause sparking between the top of the said wires and an earthed conductor across a space of 30 cm.†

The general engineering arrangements of the electric-power station erected at Poldhu for creating the electric waves of the frequency which I desired to use, were made by Dr. J. A. Fleming, F.R.S., who also devised many of the details of the appliances for producing and controlling the electric oscillations. These, together with devices introduced by me and my special system of syntonisation of inductive circuits, have provided an electric-wave generating plant more powerful than any hitherto constructed.

At the receiving station on the ship, one of my receivers, as described in the Society of Arts paper above referred to, was employed, and the signals were recorded on the tape of a Morse recording instrument.

A receiving transformer accurately tuned to the period of the electrical oscillations radiated from the transmitting station at Poldhu was connected to the coherer in the usual manner.

The receiving elevated conductor was constituted of four almost vertical wires sustained in position by the ship's mast, the summit of which wires was about 60 metres above the sea-level. At their lower end they were all connected to the receiving instrument.

My assistants at Poldhu had received instructions to send a succession of Ss and a short message at a certain pre-arranged speed, every ten minutes, alternating with five minutes of rest, during the following hours:—From 12 to 1 A.M., from 6 to 7 A.M., from 12 to 1 P.M., and from 6 to 7 P.M., Greenwich mean time, every day from the 23rd

* See 'Journal of the Society of Arts,' vol. 29, pp. 506—517.

† *Note, added* July 5, 1902. The spark-length here stated to be 30 cm. was, by a misunderstanding on the part of the communicator of the paper, altered to 50 mm., which appeared on the first proof. It was correctly stated as 30 cm. in the original MS.

February to 1st March inclusive. On board the "Philadelphia," I did not notice any apparent difference between the signals received in the day and those received at night-time, until after the vessel had reached a distance of 500 statute miles from Poldhu. At distances of over 700 miles, however, the signals transmitted during the day failed entirely, while those sent at night remained quite strong up to 1551 miles, and were even clearly decipherable up to a distance of 2099 miles from Poldhu.

It is interesting to note that at the time of the year at which these experiments took place, daylight at Poldhu was rapidly increasing between the hours of 6 and 7 A.M., and on the "Philadelphia," I noticed that at distances of over 700 miles from the sending station, the signals at 6 A.M. were quite clear and distinct, whereas by 7 A.M. they had grown weak almost to total disappearance, their strength thus apparently diminishing in proportion as daylight increased at Poldhu. No such weakening of the signals was noticeable between the hours of 12 midnight and 1 A.M.

With a view to further tests in this same connection, I carried out other experiments between the station at Poldhu and a receiving station (in all respects similar to the one on the "Philadelphia") situated at the North Haven, Poole, Dorset. The distance between the North Haven and Poldhu is about 152 statute miles, of which 109 are over sea and 43 over high land. It was found that the signals from Poldhu could be perfectly well received at the North Haven during the night when four vertical wires 12·1 metres high were used in connection with the receiving instruments, whilst, all other conditions being the same, during the day the height of the wires required to be 18·5 metres in order to receive the same signals with equal clearness.

The cause of these observed differences in the effects obtained by night as compared with those noticed by day may be due to the diselectrification of the transmitting elevated conductor, operated by the influence of daylight. The electrical oscillations in the transmitting elevated conductor may thus be prevented by the discharging influence of light from acquiring so great an amplitude as they attain during darkness.

The diselectrification of negatively charged metallic bodies by light has been noticed by many observers,* and as each alternate half-oscillation in the transmitting elevated conductor must necessarily charge it negatively, the dissipating effect of light on each alternate oscillation of the electrical wave in the transmitting wire may be sufficient to cause a material decrease in the amplitude of the oscillations.

* See papers by Messrs. Elster and Geitel in Wiedemann's 'Annalen,' pp. 38—40, also p. 497; also remarks of Professor Righi in 'Comptes Rendus,' vol. 107 p. 559.

Other tests were instituted with the object of ascertaining whether the illumination of the spark-gap of the transmitter had any effect upon the impulses transmitted, and accordingly the ball dischargers were inclosed in a box opaque to light. No perceptible difference, however, was noticed in the strength of the signals received, whether the spark-balls were or were not exposed to daylight.

It would be interesting to ascertain whether the same effects are to be observed when using transmitting elevated conductors covered with insulating material opaque to ordinary light.

I have never noticed any appreciable difference in the distances over which signals are obtainable during the day and the night respectively in the course of all the other numerous experiments which I have carried out with installations not designed for very long distances, and in which the electrical power used at the sending station has been small compared with that used at the Poldhu installation.

Probably the much higher potential to which the elevated conductor at Poldhu was charged may have greatly increased the facility with which losses might occur, due to diselectrification through the influence of daylight.

I hope to be able to make a complete study of the effects described in this note, in the course of further long-distance tests which are likely to be undertaken shortly.

"A Portable Telemeter, or Range-finder." By George Forbes, F.R.S. Received February 22,—Read March 20, 1902.

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7. The binocular.
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9. Construction and mounting of the prisms.
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11. Errors of the binoculars.
12. Practical adjustments.

(1.) *General Description.*—The instrument consists of a folding steel base, 6 feet in length, and a field glass. The base is a square tube hinged at its middle, and folds up to 3 feet 3 inches. Each half has at each end a doubly reflecting prism. The rays of light from a distant object strike the outer pair of these four prisms, are reflected at right

angles along each tube, and are then reflected at the two middle prisms into the two telescopes of the binocular fixed to the base, in directions parallel to the original rays, intercepted by the outer prisms. It is the measurement of the angle between these rays that tells the distance of the object looked at. This angle is measured by two vertical lines, one in each telescope, seen by the two eyes. One of the lines is fixed, the other moved by a micrometer screw until the two lines appear as one, while the object is seen distinctly. This gives the distance accurately to 2 per cent. even at 3000 yards. But now stereoscopic vision comes in and gives far greater accuracy. The line seems to stand out solid in space, and the slightest turn to a micrometer screw may cause the line to appear nearer or farther than the object looked at, and when the line appears to be at exactly the same distance, the micrometer reading gives the distance with an accuracy far greater than that attainable by observing the duplication of images on the retina.*

(2.) *The Adie Telemeter.*—This was the first short-base telemeter or range-finder ever supplied, but its inaccuracies were great. These inaccuracies were in part avoided in the designs of Barr and Stroud, and of Zeiss. These three are the only instruments of the kind which have ever been obtainable. In all of them a double base is used, placed at right angles to the direction of the object, having two plane reflectors at its two outer ends, and two plane reflectors at its middle point parallel to the others—all of these plane reflecting surfaces being inclined at 45° to the base. In all three instruments the two object glasses are close to the outer reflectors, the eye-piece or eye-pieces of the telescopes being at the middle of the base. The three instruments differ in the means of measuring the angle of parallax. The angle to be measured is very small, and if any one of the reflectors be twisted in the plane of vision through a small angle, the error produced is double this angle. This necessitates exact parallelism between the two reflecting surfaces in each half of the base. Such rigidity cannot be attained in a portable instrument. Nor can the warping action of the sun's rays be prevented from affecting the parallelism without diminished portability.

(3.) *Double Reflection at each end of the Base.*—I have succeeded in overcoming the necessity for extreme accuracy in the rigidity of the base by replacing the single reflectors by a double reflection at each end of each half of the base. The angle between the two surfaces producing the double reflection is maintained unalterable by grinding and polishing these surfaces on a special glass prism. I have experimented on many forms of prism, but fig. 1 is ex-extremely convenient. Light enters the first prism at the surface AB. It is reflected at the two surfaces BC, DE, which are silvered, and are

* [It has been found convenient in practice to replace the lines by balloons photographed on glass.]

inclined at about 45° to each other. It emerges at the surface AE, and suffers similar deflection by the second prism, and, if the angle between the silvered surfaces BC, DE be the same in both prisms, and if the axes of the prisms (*i.e.*, the line of contact of reflecting surfaces) be parallel, then the emerging ray must be parallel to the incident ray. If the axes of the two prisms be not exactly parallel, a rotation of one prism relatively to the other in the plane of vision, *i.e.*, in a plane approximately perpendicular to their axes, produces no error; and a rotation about any other axis, through any angle, produces an error which is only a small fraction of that angle. Consequently no absolute rigidity in the base connecting the first and second prisms is required, as is the case when there is only a single reflection at each end of the base.

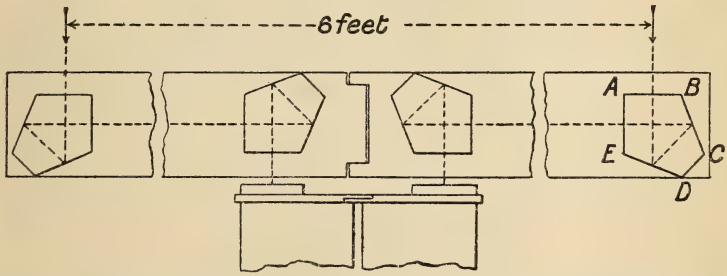


FIG. 1. Path of rays of light through base prism to binocular of range-finder.

(4.) *Advantage of placing the Object Glass outside of and behind the Base.*—In all three instruments previously perfected the object glasses are close to the outer prism. This necessitates the rigidity of the connection between the two halves of the base as well as between the reflectors constituting each half. If one-half of the base were not exactly in line with the other half, the image formed by it would be displaced relatively to the other image.

But when the base has no lens in it to form an image, if the reflecting surfaces be parallel then the emergent rays which enter the telescope must remain parallel to the incident ray, however much the base as a whole be inclined in any direction.

(5.) *The Double Base and Hinge.*—Even when the axes of the prisms are not exactly parallel the two halves do not require to be absolutely rigidly connected, and thus it has been possible to introduce a hinge which doubles the portability of the base. One-half of the base may be rotated with respect to the other about any axis.

Rotation of one-half of the base, either about the line of vision or about an axis perpendicular to the plane of vision, produces no effect except that due to shortening the base by an amount that is immaterial

in practice. Rotation of one-half of the base about the line of the base produces no error if the axes of the two prisms in each half-base be parallel, and if not parallel the hinge can easily be made sufficiently true to prevent an appreciable error arising.

(6.) *Accuracy Required.*—The angle which has to be measured is the angle between two rays from the distant object to the two ends of the base, and the distance of the object is $d = b/\alpha$ if b is length of base; and the angle corresponding to any distance is $\alpha = b/d$.

The smallest angle perceptible to the naked eye has been supposed to be 30″. A magnifying power of 12 is the highest which can be used in practice without a tripod or other stand. Hence 30″/12 = 2″·5 is the smallest angle which can be observed, or an angle = 0·0000125. Now an infantry range-finder must be capable of distinguishing between 2940 and 3000 yards (2 per cent.). Hence the angle $b/2940—b/3000$ must be at least equal to ·0000125, which gives b not less than 1·838 yards. Following this argument, I have employed a base of 2 yards' length in the form of a steel tube of square section, 0·5 mm. thick, with a hinge in the middle, so that in travelling the 6-feet base folds up to 3 feet 3 inches.

As a matter of fact I find that, owing to the adoption of stereoscopic vision, with which the limit of visibility is far less than the 30″ assumed above, the accuracy of the instrument in the hands of a practised observer on a well-defined object is far greater than 2 per cent. at 3000 yards—the limit of accuracy which I had laid down.

(7.) *The Binocular.*—When the base is stretched out and the binocular attached, the distant object can be viewed by stereoscopic vision. The eyes have little or no power of estimating absolute distance by this means, but have the greatest accuracy in the comparison of the muscular effort required to converge the eyes on objects at different distances. Two marks are fixed in the focal planes of the binocular, and it has been found best to make this mark in the form of a balloon photographed with a tail-rope hanging down to the centre of the field of view. The two balloons are almost identical, but on the left side of the left balloon the letter L is marked, and on the right side of the right balloon the letter R is marked. This assists in the focussing of each eye-piece separately, and indicates when both eyes are operative. If the distance between oculars is wrong, or if the line of the oculars be not horizontal, one of the letters, R or L, disappears.

One of the balloons is movable by a micrometer screw towards or from the other, so that they can be seen stereoscopically with any convergence of the optic axes of the eyes, and therefore at any apparent distance by turning the micrometer. The observation consists in turning the micrometer until the balloon appears to be at the same distance as the object. The micrometer head is divided and marked with distances inversely as the angular movement from infinity.

The binocular is of the prismatic type and is constructed very solidly, and the inter-objective distance must not vary much when the distance between the oculars is changed to suit different pairs of eyes. A hinged axis gives the most solid design, and the hinge is so placed as to lie on the plane containing the axes of the objectives. The distance between the objectives is then fairly constant.

The balloons would become inclined on working the binocular hinge if no special means were used to prevent this. With this object the glass photographs in the focal planes can rotate, and their mountings are provided with slides which keep them parallel. The focal length of the object-glasses is 243 mm. The pitch of the micrometer screw is 0·3 mm. The micrometer screw has a drumhead attached, 8 cm. diameter, with the periphery divided into 100 equal parts for purposes of adjustment. On the outer face a spiral groove is cut, making three and a-half turns, and a radially sliding pointer, with a pin running in the spiral groove, enables any range to be read off over three and a-half turns. The scale marked on the groove reads off distances directly, from infinity down to 500 yards.

(8.) *Advantages of Stereoscopic Vision.*—A single telescope might have been used, but the difficulty of being sure that the images are single or double is very great and leads to error. With the binocular the two images of the balloon leap together so soon as attention is concentrated on them, and the balloon seems to be at some distance; so with the distant object, however faint, the two images leap together when the attention is concentrated on them, and the object's distance can be compared with the apparent distance of the balloon. I find that I can thus appreciate an angle of 6″ with the naked eye, or 0″·5 with a power of 12.

(9.) *Construction and Mounting of the Prisms.*—The prisms are mounted by being sunk and cemented in a steel plate. The clear aperture of the transmitting surfaces is $\frac{3}{4}$-inch square. They are mounted in a tube of rectangular section which forms the base. At first I preferred aluminium, but since I was able to obtain steel tubes of the requisite quality and 0·02 inch thick I have used them. They are 1 inch high and 1·25 inch broad. The two prisms are ground and polished as one, and afterwards cut; and I find that with the special glass used the cutting does not distort the surfaces. The whole prism is included in the tube, and this can be done only by making the angle between reflecting surfaces a little less than 45°, this angle being absolutely the same in the two prisms of a pair. Each prism is held to the bottom of the square tube by three screws, which engage in the base plate of the prism, the screw heads abutting on the square tube, and the prism bed-plate being pressed from it by three pieces of clock spring with a hole through which the screw passes. Two of the screws are in line with the base tube, the third is at a right angle. Thus, if

either of the two screws first referred to be turned, the plane of the prism will be turned about a horizontal axis inclined at 45° to the base. A tilt of one prism about its axis of symmetry gives rotation to the picture about the optical axis. A tilt about an axis at right angles to the axis of symmetry raises or lowers the picture seen by the half-base under consideration.

(10.) *Errors of the Base and Hinge.*—The errors of the hinge and base may best be considered with reference to the half-base. The error is the angular deviation, measured in the plane of the binocular, of the ray entering the binocular from the direction in which it entered the outer end of the base. This error depends upon three things—(1) the inclination of the axis of the two prisms to each other, (2) the position of the plane which is parallel to these two axes relatively to the line of vision, and (3) the part of the field of view in which the observation of the distant object is made.

Let OZ be parallel to the axis of the first prism and OP parallel to the axis of the second prism. Let OPZ be the plane of XZ. Let OS be parallel to the direction of the object viewed. Let xyz, $x'y'z'$ be the co-ordinates of the point S according as OZ or OP is taken as axis of Z.

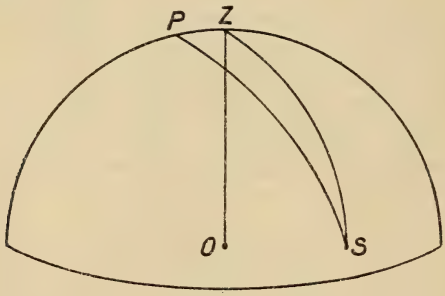


Fig. 2.

Let ZS $= \theta$, and PS $= \theta'$.

$\pi - \text{PZS} = \phi$, and ZPS $= \phi'$.

PZ $= i$, which is a very small quantity.

$\theta' = \theta + \delta\theta$, $\phi' = \phi + \delta\phi$; then $\delta\theta$ and $\delta\phi$ are very small.

$\frac{1}{2}\pi - \theta = \alpha$, then in practice α is small.

OZ = unity. Then we have

$$x = \sin\theta \,.\, \cos\phi \qquad x' = \sin\theta' \,.\, \cos\phi'$$
$$y = \sin\theta \,.\, \sin\phi \qquad y' = \sin\theta' \,.\, \sin\phi'$$
$$z = \cos\theta \qquad z' = \cos\theta'$$

$$\sin\theta'\cos\phi' = x' = x\cos i + z\sin i = \sin\theta\cos\phi\cos i + \cos\theta\sin i \ldots (1)$$

$$\sin\theta' \,.\, \sin\phi' = y' = y = \sin\theta\sin\phi \quad \ldots\ldots\ldots\ldots (2)$$

$$\cos\theta' = z' = z\cos i - x\sin i = \cos\theta\cos i - \sin\theta\cos\phi\sin i \ldots (3).$$

From (3) we have

$$\cos(\theta+\delta\theta)-\cos\theta = -\sin\theta\cos\phi\,.\,i$$

so that $$\delta\theta = i\,.\,\cos\phi \quad\text{.....}(4).$$

Now the first prism deflects the line (θ_1, ϕ_1) or (θ_1', ϕ_1') the direction of the object, to (θ_2, ϕ_2) or (θ_2', ϕ_2') the direction of the image, where $\theta_2 = \theta_1$, and the second prism deflects this line back to (θ_3', ϕ_3'), where $\theta_3' = \theta_2'$. Also, since the angle between reflecting surfaces is equal in the pair of prisms, and generally taken equal to $\frac{1}{4}\pi$, we have

$$\phi_2' - \phi_3' = \phi_2 - \phi_1 = \tfrac{1}{2}\pi \quad\text{.....}(5).$$

Now the deflection of the image vertically, in the plane through the axis of the second prism, after reflection in the two prisms, is

$$\theta_3' - \theta_1' = \theta_2' - \theta' = (\theta_2' - \theta_1) - (\theta_1' - \theta_1)$$
$$= (\theta_2' - \theta_2) - (\theta_1' - \theta_1) = \delta\theta_2 - \delta\theta_1.$$

Therefore vertical displacement $= i(\cos\phi_2 - \cos\phi_1)$ .....(6).

Also from (2)

$$\sin(\phi+\delta\phi) = \sin\phi' = \frac{\sin\theta}{\sin\theta'}\,.\,\sin\phi = \frac{\sin\theta}{\sin\theta+\delta\theta\cos\theta}\,.\,\sin\phi$$
$$= (1-\delta\theta\cos\theta)\sin\phi,$$

and therefore by (4) $\delta\phi = -\delta\theta\cos\theta\tan\phi = -i\cos\theta\sin\phi$.

The error of observation is the angular deflection of the image horizontally, supposed to be in a plane perpendicular to the axis of the second prism, after reflection in the two prisms, and is

$$\phi_3' - \phi_1' = [\text{by (5)}]\,(\phi_2' - \phi_2 + \phi_1) - \phi_1'$$
$$= \delta\phi_2 - \delta\phi_1 = -i\cos\theta\,(\sin\phi_2 - \sin\phi_1)$$

or error $$= -i\,.\,\alpha\,(\sin\phi_2 - \sin\phi_1) \quad\text{.....}(7).$$

Now, translating there, we see from (6) that the vertical displacement of the image by one-half of the base is

$$i(\cos\phi_2 - \cos\phi_1),$$

which is clearly a maximum for a fixed value of i, if $\phi_2 - \phi_1 = \frac{1}{2}\pi$, when

$$\phi_2 = \tfrac{3}{4}\pi, \quad\text{and}\quad \phi_1 = \tfrac{1}{4}\pi.$$

This occurs when the plane cutting one prism symmetrically through its axis is parallel also to the axis of the other prism; and, if I be the greatest possible inclination of the axes of the two prisms to each other, through bad adjustment, then the vertical displacement cannot exceed

$$\sqrt{2}\,.\,I \text{ for one half-base} \quad\text{and}\quad 2\sqrt{2}\,.\,I \text{ for the whole base.}$$

Again, we see from (7) that the error arising from want of adjustment of the prisms

$$= -i\,\alpha\,(\sin\phi_2 - \sin\phi_1),$$

which is clearly a maximum for a fixed value of i, if $\phi_2 - \phi_1 = \frac{1}{2}\pi$, when

$$\phi_2 = +\tfrac{1}{4}\pi, \quad \text{and} \quad \phi_2 = -\tfrac{1}{4}\pi.$$

This occurs when the plane parallel to the axes of the two prisms is at right angles to a plane cutting the prisms symmetrically.

And, if I be the greatest possible inclination of the axes of the two prisms to each other, through bad adjustment, then the error in the angle used for finding the range cannot exceed

$$\sqrt{2}\,.\,\mathrm{I}\alpha \text{ for the half-base} \quad \text{and} \quad 2\sqrt{2}\,.\,\mathrm{I}\alpha \text{ for the whole base.}$$

Also it appears that there is no error in this angle when the axes of both prisms are in one plane cutting the prisms symmetrically. Similarly, if the plane of the axes is at right angles to this plane there is no vertical displacement of the image.

It is found that with ordinary care in use the maximum value of I need never exceed one or two minutes of arc for a long period after the adjustments have been made. Now, in order to attain an accuracy of 2 per cent. (60 yards) in 3000 yards, we may not have an error greater than 2·8 seconds of arc. Hence, if I amount to 1′, we must have

$$\alpha \text{ less than } \frac{2''\cdot 8}{1' \times 2\sqrt{2}} = 0\cdot 0165 = \text{nearly } 1^\circ,$$

but the diameter of field of view of the instrument is 2°. Hence it is possible to observe the object in any part of the field of view, without error amounting to the prescribed limit, if the inclination of the axes of the prisms to each other does not exceed 1′. And by bringing the object viewed to the centre of the fields limited by the outer prisms the accuracy of the base is practically perfect.

The only defect of the hinge which can lead to error is due, as stated above, to a rotation of one-half of the base with respect to the other half of the base, about the line of the base. When this occurs, the two fields of view seen by the two eyes are not the same, although the object whose distance is required may be in both fields. The conclusions arrived at in this section prove that, if the prisms in each half base were parallel to within 1′, then, if one image can be seen in any part of its field when the other is in the centre of its field, the error is under the limits assigned, and this would involve an error of 1° in the allignment of the two halves of the base. It would be very inconvenient to have the two fields of view differing so much in appearance; and this error never amounts in practice to the prescribed limit.

(11.) *Errors of the Binoculars.*—If the binoculars be not held level, the plane in which the measurement of angles is made may be inclined to the plane of vision. The error amounts only to the versed sine of the angle of inclination, and to produce an error of only 1 per cent. in the distance, this angle would have to be at least 8°. The binocular is liable to an index error, which can be corrected by setting the index of the scale if its amount be known. This error would be a constant source of trouble if the instrument were not strongly made. Even so, however, the index must be set whenever the distance between oculars has been changed for a new observer. To test the index of the binocular, choose any object of unknown distance. First use the instrument without the base, and move the index to read infinity. Then use the base. Suppose the distance recorded to be 3000 yards. Deduct 1/30th, because the distance between the objectives is 1/30 of the whole base. Set the index so as to read 2900 yards; the index error is then corrected for all distances. Temperature may have an influence on the binocular by expanding to different extents the metal connecting the object glasses and the metal connecting the balloons in the focal planes. Most of these metal connections are a solid brass hinge which cannot be subject to variations of temperature in its parts. But the other parts of the connections, in the bodies of the telescopes, might vary one or two degrees in temperature. The distance between the centres of the objectives or balloons, independent of the hinge, is about 25 mm., but cannot be measured accurately. With brass 25 mm., each degree Centigrade expands it 0·0005 mm. Now the focal length of the objective is 143 mm., and the angular permissible error being 0·000014, the difference between the distances by which the two objectives and the two balloons are separated must not vary to the extent of 143 mm. × 0·000014 = 0·00200 mm. due to 4° C. This difference of temperature is not to be expected in practice. In cases where great distances may have to be measured with the utmost exactness the index error should be corrected, in the manner described above, at the time of observation, and this will correct for temperature. To do this easily the binocular is mounted to the base by a hinge, which enables the base to be removed during the preliminary observation with a minimum of time and trouble.

(12.) *Practical Adjustments.*—The fields of view of the binocular are limited by two round diaphragms. The outer prisms show a square field with ragged edges in the round field. The hinge of the base and the binocular mounting are so set by the maker that each square field is in the middle of its round field horizontally. They may be above or below, and they may be twisted about the visual line, and these corrections are separately made by the two adjusting screws on each of the two middle prisms. It is best to make all adjustments first on the half-base to which the binocular is attached; and afterwards on

the other half-base, using the whole base with the binocular. Having now got the square fields in the middles of the round fields, and square with each other, these adjustments seldom if ever require attention. All subsequent adjustments are made by the screws on the outer prisms; the object of these adjustments is to make the prism axes parallel. If the eyes seeing the balloons as one balloon see distant objects double, one image over the other, then the screw which tilts the prism about an axis perpendicular to its axis of symmetry will accurately remove this defect, and here also one prism is adjusted with the half-base first, and then the other prism, with the whole base. The tilting of the prism about the axis of symmetry would be cured by the other screw, but the defect is not easily seen by the binocular. It can however be detected by the collimator and removed by using the adjusting screw, which operation will not affect the previous adjustments.

The collimator is a telescope with a spectroscope-slit, near the eye end, mounted at the end of a tube projecting at right angles to the telescope tube and attached to it near the focal plane, and having a right angled total reflection prism in the axis of the telescope and a mirror with a universal joint for illuminating the slit. The binocular being removed, the collimator is pointed to one of the middle prisms. Four images of the slit are then seen, reflected from the two unsilvered surfaces of each of the two prisms in the half-base under examination. They are easily identified by their different brightnesses. The 2nd and 3rd in brightness are chosen, and the screw worked until these appear in the same line. Then the faces of the two prisms which are opposed to each other must be parallel so far as a tilt about the direction of vision is concerned. Adjustment about a vertical axis is not necessary. The two adjustments now made render the axis of the two prisms perfectly parallel, and the base is adjusted. The slit of the collimator can be varied in width by a divided screw head. One turn of the screw opens the jaws of the slit fully, when the width is three minutes of arc. This gives a ready means of measuring the error. There are other methods of proceeding to the corrections of the outer prisms. We may begin by using the collimator and adjusting either or both of the screws previously used; and afterwards, with the binocular, and using the third screw, we make the ends of the balloon ropes rest on the same distant horizontal line. In actual warfare this method has certain advantages.

Addendum, received March 1, 1902.

Beaufort West, Cape Colony.

I have made a large number of tests of the instrument, some days giving better results than others. Instead of choosing the days that

gave the most favourable results, I prefer to append to my paper the results of two days upon which I had with me experienced independent authorities, who were good enough to draw out and sign certificates of all the tests made.

Copy of certificate of an ordinary day's work on a cloudy day, drawn out and signed by Sir David Gill, K.C.B., H.M. Astronomer, Sir John Ardagh, K.C.I.E., C.B., R.E., Lieut.-Colonel J. E. Edmonds, R.E.

February 5, 1902, *Royal Observatory, Cape of Good Hope.*

The following measures of distance were made from the roof of the Royal Observatory, Cape Town, on February 5, at 4 P.M., on a dull day, clear atmosphere, light S. breeze, without tripod stand, by Professor G. Forbes. The distances recorded were in each case read independently off the screw head by Mr. V. Löwinger, of the Observatory. The actual distances of these points are based on actual survey.

	Distance in yards.	Observed.	Computed—observed.*
		yards.	yards.
Mowbray Church	1859	1868	−9
House of Refuge	3036	3127	−91
North-east corner cottages between Observatory and Salt River	852	857	−5
Chimney of Cement Works, Salt River ..	1432	1440	−8
Chimney in Salt River Railway Works ..	1711	1748	−37
Dutch Reformed Church steeple, Woodstock†	2740	2920	−180

* [The errors being all negative and increasing with the distance is due to the binocular having had its scale index set inaccurately.]

† The steeple was at the time enveloped in a dust storm.

Copy of certificate of an ordinary day's work on a very windy and dusty day, drawn out and signed by Sir David Gill, K.C.B., H.M. Astronomer.

The following measures of distance were made from the roof of the Royal Observatory, Cape Town, on February 6, at 11 A.M., bright clear day, strong S. wind, by Professor G. Forbes. The distances recorded were in each case read independently off the screw head by Mr. V. Löwinger, of the Observatory. The distances of the points are based on actual survey.

	Separate results.	Mean observed.	Actual distance.	Computed—observed.
		yards	yards.	
Mowbray Church	1860 1885 1825	1857	1859	+2
Cement Works Chimney, Salt River.....	1430 1410 1405	1415	1432	+17
Salt River Works Chimney..	1715 1735 1705	1718	1711	−7
N.E. corner of cottages between Observatory and Salt River.....	861 858 850	856	852	−4
Chimney of Oude Mollen...	701 702 711	705	686	−19
Spire at Altentby Asylum ..	751 744 733	743	749	+6
Spire at Clarendon Mowbray	1910 1950 2050	1970	1994	+24
Chimney at Nieuve Moelem	1350 1395 1380	1375	1388	+13

[Since the above was written, the instrument has been carried on horseback between 300 and 400 miles in less than three weeks with Colonel Crabbe's column in the South African War. The author frequently tested with the collimator the parallelism of the prisms in each half of the base. The error never amounted to 1 minute of arc. Applying this in the formulæ of the paper, it follows that the angle α may amount to 1° without introducing an error of $2\frac{1}{2}''$, the limit of accuracy aimed at. In other words, while it is shown in the paper that the error increases with the vertical angular distance of the object aimed at from the centre of the field, the tests during a rough experience show that the limit of accuracy aimed at is not exceeded, though the object be in any part of the field of view, and that no adjustment is required after leaving the maker's hands.]

"The Dissipation of Energy by Electric Currents induced in an Iron Cylinder when rotated in a Magnetic Field." By ERNEST WILSON, Professor of Electrical Engineering, King's College, London. Communicated by Sir W. H. PREECE, F.R.S. Received May 28,—Read June 12, 1902.

The effect which induced electric currents have upon the distribution of magnetism in an iron cylinder, when rotated in a magnetic field, has formed the subject of a communication,* and it is the object of this paper to deal with the energy dissipated by these electric currents.† A comparison will be made between the results of experiment and theory. In connection with the latter, I have to thank J. B. Dale, Esq., M.A., Assistant Lecturer in Mathematics, King's College, London, for a contribution which at his wish I append to this paper. The cylinder used in the experiments has diameter and length each 10 inches (25·4 cm.). Since the previous experiments were made additional holes have been drilled in a plane containing the longitudinal axis of the cylinder, and its present section is shown in fig. 1. Insulated copper conductors have been threaded through these holes, and inclose areas indicated by the thick lines in fig. 1. The coils are numbered 1, 2′, and 4, and inclose areas of 25·8, 232, and 654 sq. cm. respectively. As in the previous experiments, the electromotive force in each coil has been calculated from the observed deflection of the needle of a D'Arsonval galvanometer placed in each circuit. The deflections were observed simultaneously, thus enabling the relative phase-displacements to be determined.

Specific Resistance.

The specific resistance of the material of the cylinder has not been directly determined. The late Dr. John Hopkinson gives $10{\cdot}5 \times 10^{-6}$ ohm for mild steel,‡ and I have used 10×10^{-6} ohm in connection with the cylinder. A remarkable case of high specific resistance, combined with high magnetic permeability and relatively small dissipation of energy by magnetic hysteresis under alternating magnetic force, is an alloy of iron and aluminium,§ in which the specific resistance is 39×10^{-6} ohm. Provided it keeps its good qualities after prolonged use, this alloy should prove of great value.

* See Wilson, 'Roy. Soc. Proc.,' vol. 69, p. 435.

† I wish again to acknowledge the grant for the purposes of this research which was voted to me by the Council of the Royal Society out of the Government Grant.

‡ See 'Phil. Trans.,' 1885.

§ See Barrett, Brown, and Hadfield, 'Inst. Elec. Eng. Journ.,' vol. 31, part 4, pp. 681 and 709.

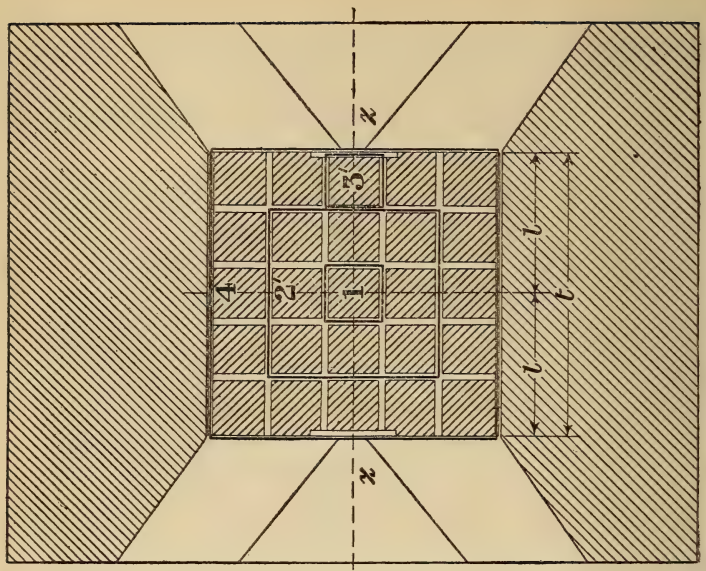


Fig. 1.

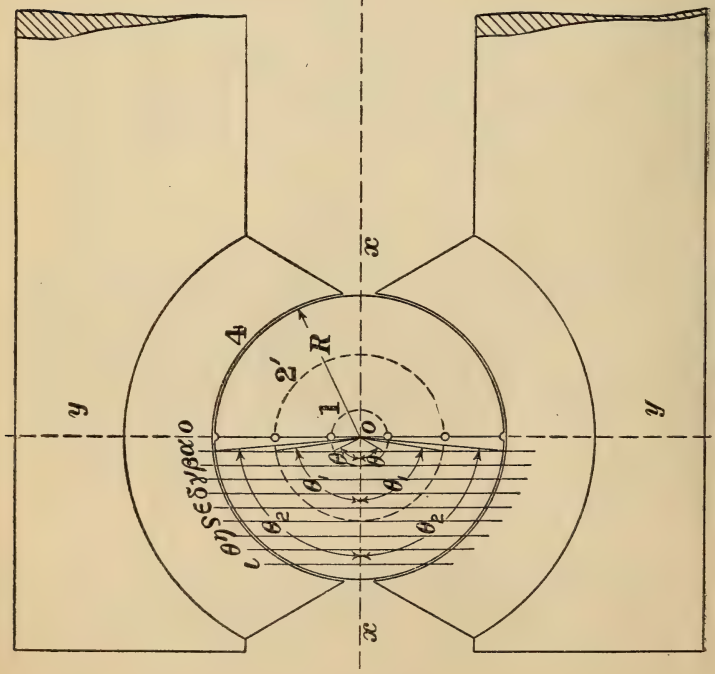


Fig. 2.

Experiment.

The rate at which energy is being dissipated by electric currents induced in the cylinder, has been obtained in the following manner. The horizontal radius in fig. 2 has been divided into ten equal parts, and the cylinder is supposed to be cut by vertical planes 0, α, β, γ . . . passing through the points of intersection, the plane 0 containing the axis of the cylinder. The circles 1, 2′, 4 show the paths traversed by the longitudinal sides of the exploring coils during rotation of the cylinder. In the plane α let θ, θ_1, θ_2 be the angles which the plane of the exploring coils makes with the horizontal when the coils 1, 2′, 4 have respectively one of their longitudinal sides in the plane α. Three paths of induced electric currents in the plane α are now assumed. No. 1 is taken to be the rectangle having its two sides, which are parallel to the longitudinal axis, equal to and coinciding with the sides of the coil, and its two other sides the straight lines necessary to complete the rectangle in the plane α. Similarly with regard to coils 2′ and 4. Each path is assumed to have a cross-sectional area of 1 sq. cm. throughout its longitudinal length, and this area multiplied by the sine of the angle which the plane of the coil makes with the horizontal, throughout the remaining portion of its length. The electric resistance of each path has been calculated in ohms. Fig. 3

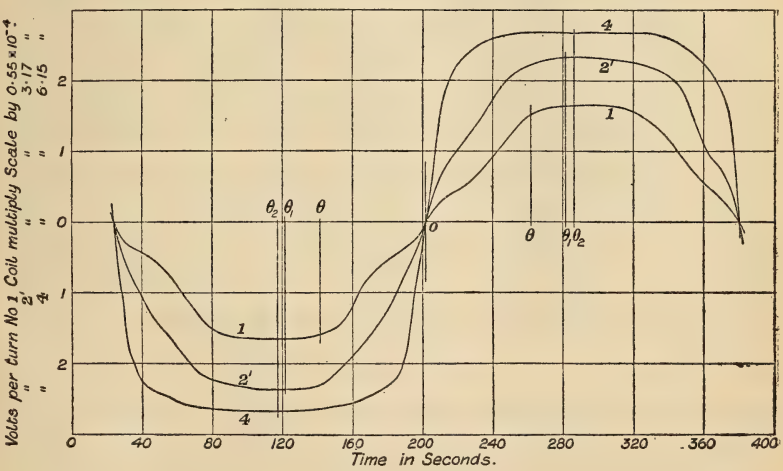


FIG. 3.

gives a set of curves obtained with 360 seconds periodic time, and maximum average induction 19,900 C.G.S. units per square centimetre for No. 4 coil. The curves give the electromotive forces in volts per turn for coils 1, 2′, and 4 respectively. The electromotive force of No. 1 coil at angles θ on each side of the point 0, where the curve

cuts the axis of time, has been halved, and the sum of the values thus obtained is taken to be the effective electromotive force in volts acting around the assumed path of the induced electric current. Similarly for coils 2′ 4 at angles θ_1 θ_2 respectively in fig. 3. The rate of dissipation of energy in watts is taken to be the square of this electromotive force in volts, divided by the resistance of the path in ohms. These watts have been plotted on squared paper, and the total watts in the plane α have been obtained by integration with respect to the vertical distance from the horizontal axis. Finally, by integrating with respect to the horizontal axis ox, the total watts dissipated in the half cylinder have been found. The figures thus obtained at periodic times, 45, 90, and 360 seconds, for different values of the total flux of induction between the pole pieces, are set forth in Table I. Strictly this method is only applicable when the electromotive forces of coils 1, 2′, and 4 are in phase. With a periodic time of 360 seconds the curves are nearly in phase for all values of the external magnetising force, but this is not the case for the other periodic times, except at the high forces. The electromotive force curves are assumed to be in phase in each experiment when applying the above process of integration.

Influence of Wave-form.

If reference be made to fig. 3 it will be seen that No. 4 coil gives an electromotive force whose wave-form lies between a sine curve and a rectangle. Suppose that the electromotive-force curve of No. 4 coil were a rectangle having the same area as a sine curve about the same base line. Since the maximum ordinate of the sine curve is $\pi/2$ of the ordinate of the rectangle, it follows that the minimum induction density in the case of the rectangle will be $2/\pi$ of the average induction density. Since also with the rectangle the electromotive force is constant for the No. 4 coil, the induction density will increase inversely as $\sin \theta_2$ (fig. 2). The value of the intensity of magnetic induction in each of the planes α, β, γ . . . has been calculated, and the graphical treatment described in connection with the experiments has been applied. It gives as a result $2{\cdot}08 \dfrac{B^2 f^2 l^2}{10^{16}\rho}$ for the watts per cubic centimetre dissipated by induced currents, the symbols having the definition given in the next section of this paper. The graphical treatment has also been applied when the intensity of magnetic induction is constant, and gives $3{\cdot}93 \dfrac{B^2 f^2 l^2}{10^{16}\rho}$ for the watts per cubic centimetre. This formula is in close agreement with the result of theory. We should expect, therefore, that the experimental results would give a less rate of dissipation of energy than dictated by the consideration of constant intensity of induction, and this is found to be the case.

It may be mentioned that as the above graphical methods rest upon calculation, the paths of the respective curves were determined by many more points than were possible in connection with the experimental results.

Theoretical Considerations.

Let B = average induction per square centimetre in C.G.S. units.
f = frequency in complete periods per second.
ρ = specific resistance of the material of the cylinder in ohms.
$t = 2l$ = length of cylinder in centimetres along its longitudinal axis.
$2nl$ = diameter of cylinder in centimetres.
R = maximum radius of cylinder in centimetres.
x, y, z be axes as shown in figs. 1 and 2.

In any plane section such as α, fig. 2, one can consider three distributions of the currents induced in the cylinder by rotation in a magnetic field, the lines of force being parallel to the planes α, β, γ, . . . and at right angles to the longitudinal axis of the cylinder.

(1.) There is the distribution assumed by Baily* in which the electric currents flow in rectangular paths similar to the boundary of the plane of section $\alpha\beta\gamma$ . . . . On the assumption that the rate of cutting lines of force is proportional to the distance from the centre of a plate, and that the distribution of magnetism is such that any point in the plate is cutting lines of force during two-thirds of a revolution, and that for the remaining one-third revolution the point is travelling along the lines of force, Baily has shown that, when R is great compared with t, the average rate of dissipation of energy per cubic centimetre of the plate is given in watts by the formula $3\dfrac{B^2f^2t^2}{10^{16}\rho}$. Assume that the cylinder rotates about its longitudinal axis in a truly uniform magnetic field. Assume also with Baily that the length of the path of the induced current

$$= 4z + 4nz \sin\theta_2 = 4z\left\{1 + n\frac{\sqrt{(n^2l^2 - x^2)}}{nl}\right\} \text{cms.},$$

and that the electric resistance of the path

$$= \frac{4z\rho}{dx\,dz}\left\{\frac{1}{n} + n\frac{\sqrt{(n^2l^2 - x^2)}}{nl}\right\} \text{ ohms.}$$

The electromotive force in volts $= 8z^2\pi fn\mathrm{B}10^{-8}$. Then the total rate of dissipation of energy in watts

$$= \frac{16\pi^2\mathrm{B}^2f^2n^3l}{10^{16}\rho}\int_0^{nl}\frac{lx}{l + n\sqrt{(n^2l^2 - x^2)}}\int_0^l z^3\,dz,$$

* See 'Phil. Trans.,' A, vol. 187 (1896), pp. 715—746.

and this reduces to

$$\frac{8B^2f^2l^5\pi^2}{10^{16}\rho}\left(\frac{\pi}{2}-1\right), \text{ when } n = 1;$$

$$\frac{8B^2f^2l^5n^3\pi^2}{10^{16}\rho}\left(\frac{\pi}{2n}-\frac{1}{n\sqrt{(n^4-1)}}\log\frac{\sqrt{(n^2+1)}+\sqrt{(n^2-1)}}{\sqrt{(n^2+1)}-\sqrt{(n^2-1)}}\right), \text{ when } n \text{ is greater than unity;}$$

$$\frac{8B^2f^2l^5n^3\pi^2}{10^{16}\rho}\left\{\frac{\pi}{2n}-\frac{2}{n\sqrt{(1-n^4)}}\left(\tan^{-1}\sqrt{\frac{(1-n^2)}{(1+n^2)}}\right)\right\} \text{ when } n \text{ is less than unity.}$$

The average watts per cubic centimetre of the cylinder as given by the above formulæ are

$$7\cdot16\,\frac{B^2f^2l^2}{10^{16}\rho} = 1\cdot79\,\frac{B^2f^2l^2}{10^{16}\rho} \text{ when } n = 1.$$

$$19\cdot7\,\frac{B^2f^2l^2}{10^{16}\rho} = 4\cdot9\,\frac{B^2f^2l^2}{10^{16}\rho} \text{ when } n = 200.$$

When $n = \frac{1}{200}$ the value of $\frac{\pi}{2n} - \frac{2}{n\sqrt{(1-n^4)}}\left(\tan^{-1}\sqrt{\frac{1-n^2}{1+n^2}}\right)$ is very small indeed.

By modifying the expression for the resistance of the rectangular path, Mr. Dale shows that the watts per cubic centimetre $= 4\cdot69\,\frac{B^2f^2l^2}{10^{16}\rho}$ when $n = 1$, and $12\cdot6\,\frac{B^2f^2l^2}{10^{16}\rho}$ when n is much greater than unity.

(2.) There is the distribution assumed in connection with the experiments, that is, the induced currents distribute themselves on the surfaces of cylinders similar to and concentric with the cylinder experimented upon. Mr. Dale has dealt with this case theoretically for a uniform magnetic field, and shows that the watts per cubic centimetre of the cylinder $= 3\cdot95\,\frac{B^2f^2l^2}{10^{16}\rho}$ when $n = 1$, and $7\cdot9\,\frac{B^2f^2l^2}{10^{16}\rho}$ when n is much greater than unity. The former coefficient is in very good agreement with the graphical result, which is 3·93.

(3.) There is the case in which the current density in any path is constant throughout the whole length of the path. Mr. Dale has also dealt with this case theoretically and shows that the watts per cubic centimetre $= 4\cdot09\,\frac{B^2f^2l^2}{10^{16}\rho}$ when $n = 1$, and $11\cdot1\,\frac{B^2f^2l^2}{10^{16}\rho}$ when n is much greater than unity.

Comparison of Experiment with Theory.

In Table I the formula $3{\cdot}95 \frac{B^2 f^2 l^2}{10^{16}\rho}$, multiplied by the volume of the half cylinder, is used, in which B is the maximum average value of the intensity of magnetic induction given by coil No. 4. It will be seen that experiment gives a lower value for the total watts in the half cylinder than the formula, and this is to be expected from the wave-form.

At 45 seconds periodic time and maximum average induction density for No. 4 coil 5900, the ratio of the watts given by theory and experiment is 3·15, which is much larger than the average. The explanation is that the magnetic induction is crowded to the surface of the cylinder, and as the wave-form for this portion is more nearly rectangular, the result is to make the watts given by experiment relatively smaller. At periodic time 90 seconds we see the same influence at work when maximum average induction density given by coil 4 is 6700. On the other hand, there is no great difference between the results at 360 and 45 seconds periodic time with approximately uniform distribution of the intensity of magnetic induction.

Alternating Magnetic Force.

Fleming* gives the expression $0{\cdot}616 \frac{B^2 f^2 d^2}{10^{16}\rho}$ in a uniform magnetic field for the watts dissipated by induced currents per cubic centimetre of a cylinder to which an alternating magnetic force is applied whose direction is parallel to the longitudinal axis of the cylinder. The diameter of the cylinder is d centimetres, and the other symbols represent the same quantities as before. This formula for frequency 129 and $\rho = 13 \times 10^{-6}$ ohm is approximately verified by experiment. The following results were obtained with a cylinder of mild steel 4 inches (10·16 cm.) diameter,† and have been extended to the case of a cylinder 0·1 cm. diameter on the assumption that similar electric and magnetic events happen in cylinders of different diameters at times varying inversely as the square of their diameters :—

* See 'The Alternate Current Transformer,' vol. 2, p. 487.

† See Hopkinson and Wilson, 'Phil. Trans.,' A, vol. 186 (1895), pp. 93–121.

Maximum average B at centre of cylinder.	Maximum average B over whole cylinder.	Frequency with the cylinder 10·16 cm. diameter.	Ergs per cycle per cubic centimetre.	Frequency with a cylinder 0·1 cm. diameter.	Watts per cubic centimetre of a cylinder 0·1 cm. diameter.	Fleming's formula $d=0{\cdot}1$ cm.
7690	6888	1/80	3820	129	0·049	0·037
15000	17050	,,	26200	,,	0·338	0·222
1630	4710	1/20	2750	516	0·142	0·282
15500	20000	,,	54500	,,	2·810	5·07

Turning from round wires to plates, in which the cross-section normal to the lines of force has a length great as compared with its thickness, Fleming,* Steinmetz,† and Thomson‡ agree in giving $1{\cdot}66\,\frac{B^2f^2t^2}{10^{16}\rho}$ for the watts per cubic centimetre. A plate would, therefore, dissipate nearly 2·7 times as much energy in a given time as a wire having a diameter equal to the thickness of the plate.

Rotating and Alternating Magnetic Fields.

We have seen that the results of theory are fairly well verified by experiment. It remains to extend the formulæ to cases actually met with in the construction of dynamo electric machinery. It is reasonable to assume that the electric currents distribute themselves in paths such that constant current density is preserved. The formula $2{\cdot}78\,\frac{B^2f^2t^2}{10^{16}\rho}$ may be taken to apply with a good deal of certainty in the case of circular plates in which the diameter is great as compared with the thickness, and in which the lines of force are uniformly distributed in the plane of the plate. Comparing this with the formula $1{\cdot}65\,\frac{B^2f^2t^2}{10^{16}\rho}$ for alternating magnetic force, we may say that the rotating field would dissipate about 1·7 times as much energy in a given time as the alternating magnetic field. It should be remembered, however, that even when the lines of force are confined to the plane of the plate, the dissipation of energy for a given average induction density may be considerably reduced if the distribution is such as to give the induced electromotive force a more rectangular wave-form. Some time ago§ I tried to account for the loss in a smooth-core drum armature, in which the magnetic field was distributed

* See 'The Alternate Current Transformer,' vol. 2, p. 490.

† See Wiener's 'Dynamo-electric Machines,' p. 119.

‡ See 'Phil. Trans.,' A, vol. 187 (1896), p. 723.

§ See 'The Electrician,' 11th October, 1895.

Table I.

Periodic time in seconds.	Frequency for a cylinder having diameter and length = 0·1 cm.	Maximum average induction per square centimetre B.				Phase difference between E.M.F. curves. 360° = 1 period.			Watts dissipated by induced currents in half volume of the cylinder.			
		Coil 1.	Coil 2′.	Coil 3′.	Coil 4.	Nos. 1 and 3′.	Nos. 4 and 1.	Nos. 4 and 2′.	Calculated from $2\cdot08\,\frac{\pi B^2 f^2 l^5}{10^{16}\rho}$ No. 4 coil E.M.F., rectangular.	Obtained from experiment.	Calculated from $3\cdot95\,\frac{\pi B^2 f^2 l^5}{10^{16}\rho}$	Ratio A/B.
										B	A	
45	1434	429	541	1546	1090	65°·6	73°·7	28°·8	0·0127	0·0137	0·0240	1·75
,,	,,	908	3070	7860	5900	121°	108°	59°·4	0·373	0·224	0·705	3·15
,,	,,	12980	12400	12000	12600	28°	17°·6	8°	1·70	2·280	3·22	1·41
,,	,,	18200	20000	—	20000	—	0	0	4·28	5·8	8·10	1·40
90	717	730	895	1400	1060	44°·8	39°·2	16°	0·0030	0·0036	0·0057	1·58
,,	,,	2660	5100	8100	6700	96°	82°·5	36°·8	0·119	0·138	0·227	1·64
,,	,,	13100	13400	13800	13300	5°·8	4°·8	0	0·472	0·708	0·895	1·26
,,	,,	17900	19300	17000	19300	0	0	0	0·993	1·41	1·88	1·33
360	179	1520	1320	—	1040	—	8°	0	0·00018	0·00027	0·00034	1·27
,,	,,	8400	8330	—	7300	—	0	0	0·0088	0·0135	0·0168	1·24
,,	,,	13740	14100	13400	14400	1°·6	0	0	0·0346	0·045	0·0655	1·45
,,	,,	20000	20600	—	19900	—	0	0	0·0662	0·0974	0·125	1·28

approximately as in these experiments. Bringing to bear the results of Professor Baily's experiments* on the dissipation of energy by magnetic hysteresis in rotating magnetic fields and the results arrived at in this paper, I am still unable to account for the observed loss on the assumption that the plates are insulated from one another. In dynamo-electric machines, where approximate uniform rotating fields are met with, the construction does not lend itself to such perfect conditions as, for instance, in the case of alternate current transformer cores. It is doubtful if the lines of force are always confined to the plate. If one considers the section of an armature core in which ventilating spaces are provided, it is highly probable that at the ends of the core and at each ventilating space there is a considerable axial component, which would at once give rise to a serious increase in the dissipation of energy by induced currents. The teeth are sometimes filed out after placing the plates in position, and this may lead to considerable conductivity normal to the plane of the plate. Then, again, the plates are not always insulated from the shaft or spider which supports them. In multipolar dynamos it is questionable if one ever meets with a fair approximation to a uniform rotating field.

Longitudinal Variation of Intensity of Magnetic Induction.

In the previous paper it was stated that the intensity of magnetic induction diminishes in value and suffers retardation in phase as one proceeds from either end of the cylinder along its longitudinal axis towards the centre, in much the same way as was observed radially in a plane at right-angles to the longitudinal axis and midway between the ends of the cylinder. This statement rested upon a comparison of the electromotive force of coil I with that of coil 1. In the present experiments an opportunity to examine this further was given, and coil 3′ (fig. 1) was wound round an area 22·6 sq. cm. at the end of the cylinder. The maximum values of the induction density over this area are given in Table I, so that a comparison can be made with the maximum value of the intensity of induction at the centre (coil 1). The phase-displacements are also given. The results further confirm the statement made in the previous paper.

Conclusion.

In my former paper I omitted to thank Professor Capper for the loan of the worm and worm-wheel used in these experiments. I have pleasure in doing so now. I have also pleasure in acknowledging the help I have received from Mr. F. S. Robertson, and Messrs. M. S. Duffitt and R. M. Wartze. I wish to mention the intelligent interest taken by Mr. Duffitt in the theoretical part of the work, and the patience and care Mr. Wartze has given to the working out of results. Mr. T. Jones has again rendered valuable assistance.

* See 'Phil. Trans.,' A, vol. 187 (1896), p. 723.

Theoretical Appendix. By J. B. DALE, M.A.

1. The rotation of a conducting sphere in a magnetic field is discussed by J. J. Thomson ('Researches in Electricity,' p. 546).

When the sphere is rotating about the axis of z, in a uniform magnetic field of force B parallel to x, the values of the current components when $\sqrt{(\mu\omega/\sigma)}\, a$ is very small are,

$$p = -\tfrac{3}{2}\frac{\mu\omega}{(\mu+2)\sigma} z\mathrm{B}, \qquad q = 0, \qquad r = \tfrac{3}{2}\frac{\mu\omega}{(\mu+2)\sigma} x\mathrm{B},$$

ω being the angular velocity and a the radius. In the case of an iron sphere we may take $\mu = 10^3, \sigma = 10^4$, and the current components are approximately

$$-\tfrac{3}{2}\frac{\omega}{\sigma} z\mathrm{B}, \qquad 0, \qquad \tfrac{3}{2}\frac{\omega}{\sigma} x\mathrm{B}.$$

The currents therefore flow in circles in planes parallel to yz, having their centres on the axis of y.

Integrating round a circle the value of the E.M.F. is found to be $3\pi\rho^2\omega\mathrm{B}$. This is likewise the value of the E.M.F. found on the assumption that the circuit is insulated and rotating in a field of magnetic induction equal to that in the sphere at rest.

The rate of heat production

$$= \sigma \iiint (p^2 + r^2)\, dx\, dy\, dz,$$

taken throughout the volume of the sphere.

Transforming to polar co-ordinates this becomes

$$\tfrac{9}{4}\frac{\omega^2}{\sigma}\mathrm{B}^2 \int_0^a r^4 dr \int_0^\pi \sin^3\theta\, d\theta \int_0^{2\pi} d\phi = \tfrac{6}{5}\frac{\pi a^5 \omega^2 \mathrm{B}^2}{\sigma}.$$

Writing for ω, $2\pi f$ where f is the frequency, we find for the rate of heat production per cubic centimetre,

$$3\cdot 95\,\frac{a^2 f^2 (3\mathrm{B})^2}{\sigma},$$

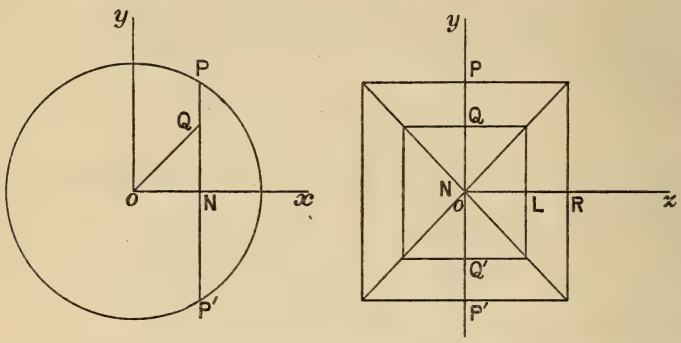
where 3B is the induction when the sphere is stationary.

2. In the case of a cylinder rotating about its axis of figure in a uniform magnetic field at right angles to the axis, we may assume, in analogy with the result for a sphere, that the induced currents circulate in planes parallel to the axis and the magnetic force, and that the E.M.F. in any circuit is the same as that which would be induced in the circuit supposed insulated and rotating in the magnetic field which exists in the cylinder at rest.

The form of the stream lines in any plane section is unknown. It is reasonable to assume that they are approximately rectangular.

Different assumptions may be made regarding the law of formation of the rectangular circuits. We shall assume, (1) that in any section the currents flow in rectangles similar to the boundary of the section; (2) that the currents flow on the surfaces of cylinders similar to the cylinder; (3) that the current density in any circuit is constant throughout that circuit; and compare the results obtained for the value of the rate of heat production according as one or other of these hypotheses is adopted.

3. Take axes through the centre of the cylinder, O_z the axis of rotation, O_y parallel to the magnetic induction in the cylinder.



Let a cm. be the radius, and $2ma$ cm. the length of the cylinder, $2\pi f$ the angular velocity. In a section PNP′ distant x cm. from the axis, assume that the currents flow in circuits similar to the boundary of the section.

Let $\mathrm{NL} = z$ cm., then $\mathrm{NQ} = z\sqrt{\dfrac{(a^2-x^2)}{ma}}$ cm.

If B is the magnetic induction, and ρ the specific resistance of the material in ohms, the E.M.F. in the circuit QLQ′

$$= 2\pi f\mathrm{B}\,.\,4z^2\,\frac{\sqrt{(a^2-x^2)}}{ma}\,.\,10^{-8}\text{ volt,}$$

and the resistance

$$= 4\rho\left(z\,\frac{\sqrt{(a^2-x^2)}}{ma} + z\,\frac{ma}{\sqrt{(a^2-x^2)}}\right)\Big/\delta x\;\delta z.$$

Hence the rate of dissipation of energy in watts per cubic centimetre

$$= \frac{16\pi f^2\mathrm{B}^2 10^{-16}}{\rho m^2 a^4}\int_0^a dx\int_0^{ma} dz\,\frac{z^3(a^2-x^2)^{\frac{3}{2}}}{[(1+m^2)a^2-x^2]}.$$

This may be integrated by substituting $a\sin\theta$ for x, and the result is

$$\frac{f^2\mathrm{B}^2 10^{-16}}{\rho}\,\pi a^2 m^2\left[4 - \frac{2m^2}{\sqrt{(m^2+1)}}\log\frac{\sqrt{(m^2+1)}+1}{\sqrt{(m^2+1)}-1}\right].$$

When $m = 1$, this is equal to

$$4{\cdot}694\,\frac{f^2B^2 10^{-16} a^2}{\rho}.$$

When m is small, the expression tends to

$$12{\cdot}6\,\frac{f^2B^2 10^{-16} a^2 m^2}{\rho}.$$

4. When the currents are assumed to flow in cylindrical shells similar to the original, let $2z$ cm. be the length. The radius of the shell $= z/m$ cm.

The thickness of the plane end is δz, and of the curved surface $\frac{1}{m}\delta z$.

Denoting by ϕ the angle xOQ, fig. (i), the E.M.F. round the elementary circuit

$$= 2\pi f B 10^{-8} \,.\, 4\,\frac{1}{m}\,z^2 \sin\phi.$$

The resistance of the circuit

$$= \left(\frac{4{\cdot}3m^2}{3\delta\phi\delta z} + \frac{4m_3 \sin\phi}{m_3\delta\phi\delta z \sin\phi}\right)\rho = \frac{4(m^2+1)}{\delta\phi\delta z}\,\rho.$$

The dissipation of energy in watts per cubic centimetre is

$$\frac{16\pi f^2 B^2 10^{-16}}{m^3(m^2+1)a^3\rho}\int_0^{\frac{1}{2}\pi}\sin^2\phi d\phi \int_0^{ma} z^4 d_2 = \frac{4\pi^2}{5(m^2+1)}\,m^2a^2\,\frac{f^2B^2 10^{-16}}{\rho}.$$

When $m = 1$, this is equal to

$$\frac{2\pi^2}{5}a^2\frac{f^2B^2 10^{-16}}{\rho},$$

or

$$3{\cdot}95a^2\,\frac{f^2B^2 10^{-16}}{\rho}.$$

When m is very small the value is approximately

$$7{\cdot}9m^2a^2\,\frac{f^2B^2 10^{-16}}{\rho}.$$

5. When the current in any section is assumed to be of uniform density throughout, the rectangular circuit must be such that QL being this circuit QP = LR, fig. (ii).

The area enclosed by the circuit is

$$4z(\sqrt{(a^2-x^2)} - ma + z)\ (\alpha), \quad \text{or} \quad 4y(ma - \sqrt{(a^2-x^2)} + y)^2\ (\beta),$$

and the length

$$4(2z + \sqrt{(a^2-x^2)} - ma)\ (\alpha), \quad \text{or} \quad 4(2y + ma - \sqrt{(a^2-x^2)})\ (\beta),$$

the form α or β being taken according as ma is less or greater than $\sqrt{(a^2-x^2)}$.

Hence the rate of dissipation of energy in watts per cubic centimetre is A + B, where

$$A = \frac{16\pi f^2 B^2}{ma^3\rho 10^{16}} \int_0^{a\sqrt{(1-m^2)}} dx \int_0^{ma} \frac{(\sqrt{(a^2-x^2)}-ma+z)^2 z^2}{(\sqrt{(a^2-x^2)}-ma+2z)} dz,$$

and
$$B = \frac{16\pi f^2 B^2}{ma^3\rho 10^{16}} \int_{a\sqrt{(1-m^2)}}^{a} dx \int_0^{\sqrt{(a^2-x^2)}} \frac{(ma-\sqrt{(a^2-x^2)}+y)^2 y^2}{(ma-\sqrt{(a^2-x^2)}+2y)} dy.$$

When m is greater than unity the integral B alone is taken, the limits of x being 0 and a.

To integrate A with respect to z, let

$$\sqrt{(a^2-x^2)}-ma = 2D \quad \text{and} \quad z+D = \xi.$$

Then

$$\int_0^{ma} \frac{(\sqrt{(a^2-x^2)}-ma+z)^2 z^2}{(\sqrt{(a^2-x^2)}-ma+2z)} dz = \int_D^{ma+D} \frac{(\xi^2-D^2)^2 d\xi}{2\xi} = \tfrac{1}{2}\left[\frac{\xi^4}{4} - \xi^2 d^2 + d^4 \log \xi\right]_D^{ma+D},$$

or, restoring the values of D and ξ,

$$\frac{1}{2^4} ma\,(\sqrt{(a^2-x^2)})\,(4ma\sqrt{(a^2-x^2)} - m^2a^2 - (a^2-x^2)) \quad \ldots\ldots \text{(C)}$$

$$+ \frac{1}{2^5}(\sqrt{(a^2-x^2)}-ma)^4 \log \frac{\sqrt{(a^2-x^2)}+ma}{\sqrt{(a^2-x^2)}-ma} \quad \ldots\ldots\ldots \text{(D)}.$$

A similar substitution in B yields as the result of integration, with respect to z,

$$\frac{1}{2^4} ma\sqrt{(a^2-x^2)}\,(4ma\sqrt{(a^2-x^2)} - m^2a^2 - (a^2-x^2)) \ldots\ldots\ldots \text{(C}'\text{)}$$

$$+ \frac{1}{2^5}(ma-\sqrt{(a^2-x^2)})^4 \log\left(\frac{ma+\sqrt{(a^2-x^2)}}{ma-\sqrt{(a^2-x^2)}}\right) \ldots\ldots\ldots \text{(D}'\text{)}.$$

Since (C) and (C′) are identical in form, we can combine them, and integrate with respect to x from $x = 0$ to $x = a$.

Let $x = a\sin\theta$, and denoting $\cos\theta$ by c and $\sin\theta$ by s, we obtain

$$\int_0^a (C+C')\,dx = \frac{ma^5}{2^4}\int_0^{\pi/2} c^2(4mc - c^2 - m^2)\,d\theta = \frac{ma^5}{2^4}\left(\frac{8}{3}m - \frac{(3+4m^2)\pi}{16}\right) \quad \ldots\ldots \text{(E)}.$$

By the same substitution D and D′ assume the forms

$$D = \frac{a^5}{2^5}\int_0^{\cos^{-1}m} c\,(c-m)^4 \log\frac{c+m}{c-m}\,d\theta \ldots\ldots\ldots\ldots\ldots\ldots \text{(F)},$$

$$D' = \frac{a^5}{2^5}\int_{\cos^{-1}m}^{\pi/2} c(m-c)^4 \log\frac{m+c}{m-c}\,d\theta \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots \text{(G)},$$

where the limits in D′ when $m>1$ are 0 and $\pi/2$. Taking this case first and integrating by parts,

$$D' = \frac{a^5}{2^5}\Big[(sm^4 - 2(cs+\theta)m^3 + 2(c^2s+2s)m^2 - \tfrac{1}{2}(2c^3s+3cs+3\theta)m$$
$$+\tfrac{1}{15}(3c^4s+4c^2s+8s))\log\frac{m+c}{m-c}\Big]_0^{\pi/2} \quad\ldots\ldots\text{(H)},$$
$$+\frac{ma^5}{2^4}\int_0^{\pi/2}\frac{s}{m^2-c^2}\,[(m^4+4m^2+\tfrac{8}{15})s - m(2m^2+\tfrac{3}{2})cs$$
$$+(2m^2+\tfrac{4}{15})c^2s - mc^3s + \tfrac{1}{5}c^4s$$
$$-m(2m^2+\tfrac{3}{2})\theta]\,d\theta\ldots\ldots\text{(K)}.$$

The integrated part vanishes at the limits, and the remainder on integration becomes

$$\frac{ma^5}{2^4}\Big[(m^4+4m^2+\tfrac{8}{15})\frac{\pi}{2m}(m-\sqrt{(m^2-1)})$$
$$-m(2m^2+\tfrac{3}{2})\left(1-\sqrt{(m^2-1)}\tan^{-1}\frac{1}{\sqrt{(m^2-1)}}\right)$$
$$+(2m^2+\tfrac{4}{15})\frac{\pi}{4}(m-\sqrt{(m^2-1)})^2$$
$$-m\left(-\tfrac{1}{3}+m^2\left(1-\sqrt{(m^2-1)}\tan^{-1}\frac{1}{\sqrt{(m^2-1)}}\right)\right)$$
$$+\frac{\pi}{80}([m-\sqrt{(m^2-1)}]^4+2[m-\sqrt{(m^2-1)}]^2)$$
$$-2(2m^2+\tfrac{3}{2})\left(k-\frac{k^3}{3^2}+\frac{k^5}{5^2}-\ldots\right)\Big] \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\text{(L)}$$

where $k = m - \sqrt{(m^2-1)}$.

In the particular case when $m = 1$, the value is—

$$\frac{16\pi f^2B^2 10^{-16}a^5}{a^3\rho 2^4}\Big[\frac{8}{3}-\frac{7\pi}{16}+\frac{83\pi}{30}-\frac{7}{2}+\frac{17\pi}{30}-\frac{2}{3}+\frac{3\pi}{80}$$
$$-7\left(1-\frac{1}{3^2}+\frac{1}{5^2}+\frac{1}{7^2}+\ldots\right)\Big].$$

The value of the series $1-\frac{1}{3^2}+\frac{1}{5^2}-\frac{1}{7^2}+-$ can be found to be 0·9160. Hence we obtain for the number of watts—

$$4{\cdot}095\,\frac{f^2B^2a^2 10^{-16}}{\rho}.$$

The integration of F and G when $m < 1$ is troublesome, but when m is sufficiently small, we may without much error neglect G, and integrate F from 0 to $\pi/2$.

$$\frac{a^5}{2^5}\int_0^{\pi/2} c(c-m)^4 \log\frac{c+m}{c-m}\,d\theta = \frac{a^5 m}{2^4}\int_0^{\pi/2} c^4\left(1 - 4\frac{m}{c} + \frac{19}{4}\frac{m^2}{c^2} - \frac{16}{3}\frac{m^3}{c^3} + \ldots\right)d\theta$$

$$= \frac{a^5 m}{2^4}\left[\frac{3}{16}\pi - \frac{8}{3}m + \frac{19}{16}m^2\pi - \frac{16}{3}m^3 - \ldots\right] \ldots\ldots\ldots \text{(M)}.$$

Adding (E) and (M) we obtain

$$\frac{9}{8}\pi^2 a^2 m^2 \frac{f^2 B^2 10^{-16}}{o} = 11{\cdot}1\, a^2 m^2 \frac{f^2 B^2 10^{-16}}{\rho}.$$

"On Skin Currents.—Part III. The Human Skin." By AUGUSTUS D. WALLER, M.D., F.R.S. Received March 17,—Read April 24, 1902.

(From the Physiological Laboratory of the University of London.)

The purport of the following observations was to examine on human skin the electrical reactions described in previous communications in the case of frog's skin, and in that of the cat's skin.

The Main Facts.—The phenomena are in the main similar under similar conditions, viz., in the case of a freshly removed healthy skin obtained from the operating theatre, the normal current is always ingoing, and the normal response is always outgoing.*

In a series of observations—with values noted for an ingoing current of between 0·0020 and 0·0200 volt—the response has appeared to be quite independent of the magnitude of normal current, and to strong stimulation has usually been from 0·0100 to 0·0400 volt.

As in the case of the frog's skin, the response to single induction shocks has been in the positive or outgoing direction, to both directions of exciting current. Tetanising currents (make and break induction shocks of alternating direction at a frequency of about 60 per sec.) give therefore a summated positive effect. At the outset of my experiments on human skin I encountered doubt and difficulty by reason of the fact that I set myself the difficult problem of determining as far as possible its duration of survival, and by so doing had to deal with cases where a reaction of low voltage is apt to be masked by admixture of polarisation currents. In my experience, healthy skin from the operating theatre, tested within 48 hours after operation, has always manifested indubitable signs of life, and by indubitable I mean a

* 'Roy. Soc. Proc.,' 1901, vol. 68, p. 480; vol. 69, p. 171.

reaction greater than + 0·0050 volt to strong induction shocks of + or − direction.*

Tested by tetanising currents of alternating directions such skin gives to both pairs of directions a similar indubitable positive response of, *e.g.*, 0·0200 to 0·0500 volt.

Moribund skin and skin obtained from the *post-mortem* room, from subjects that have died gradually, have generally afforded doubtful results, and by doubtful results I mean a reaction of variable direction and measured by ten thousandths of a volt.

In all cases the electrodes alone were carefully tested for polarisation, anomalous as well as ordinary, before use and after experiment; the skin was subsequently killed by boiling and the experiment repeated.

The course of events will be best set forth by the detailed account of a single observation.

(Observation 2.) Skin of breast, removed for carcinoma, $1\frac{1}{2}$ hours after operation. The single break induction shock of + and − direction gave the response + 0·0180 + 0·0230, the resistance being 1 megohm.

An Illustrative Observation.—To tetanisation of both pairs of directions the responses were + 0·0440 and + 0·0460. R now $\frac{1}{2}$ megohm. After boiling the resistance was only 50,000 ohms.

The next day the reactions of a fresh piece of same skin to strong single shocks + and − were + 0·0050, + 0·0175 volt. On the 4th day the reactions were + 0·0025 and + 0·0035. In all these cases the positive response was abolished by boiling.

On the 7th day the reactions were doubtful, except in the case of the nipple, where the response to all kinds of excitation was about + 0·0050. In this case the conductivity was increased from 100 to 121 by tetanisation.

Diminution of Resistance.—A remarkable feature noticed in the outset of these experiments was the great diminution of resistance caused by tetanisation. The alteration of resistance was most pronounced in the case of skin which, judging by its response, was most alive; it was far less noticeable in the case of moribund and doubtful skin; it was not apparent at all in the case of skin certainly killed by boiling, as might be expected; however, the resistance of boiled skin was always far below that of the same skin previous to boiling.

All these points will be most clearly apparent by reference to fig. 1.

The noteworthy points are—

(1.) The contrast between the four responses of the living and dead skin.

* Throughout the present paper + signifies "outgoing" and − "ingoing," as regards direction of current through the skin.

(2.) The augmented conductivity of the living skin from (12·5) 25 to 35 in consequence of tetanisation.

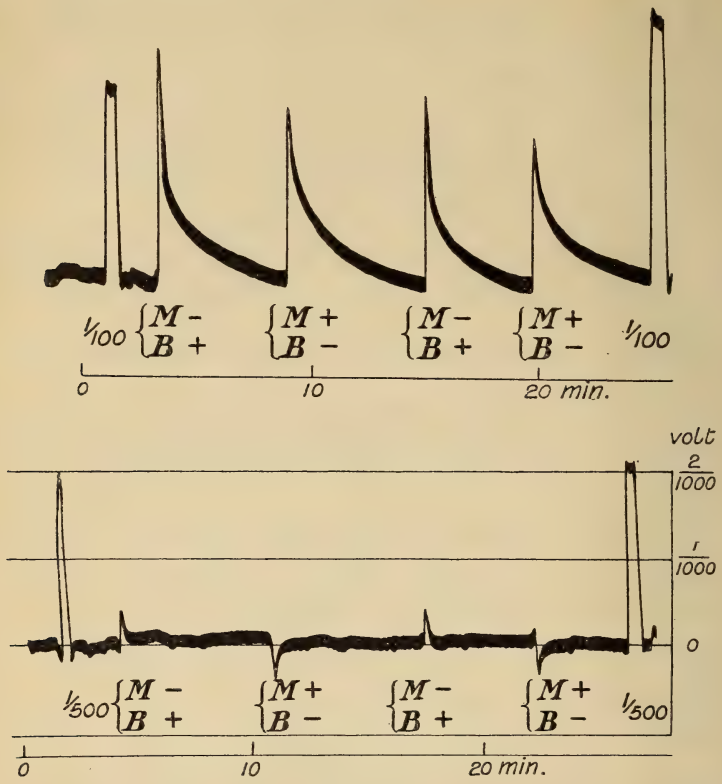


FIG. 1 (Nos. 4199—4200).—Skin of Breast 5 hours after amputation. Tested by tetanising currents for periods of 5 secs. each from a Berne coil at 5000 units, supplied by 8 Leclanché cells. Conductivity at outset of experiment = 5, and calculated resistance = 560,000 ohms. After tetanisation the conductivity was raised to 12·5 (= 224,000 ohms; after further tetanisation it rose further to 25 (= 112,000 ohms), when the record commenced.

A, living.—Conductivity at outset ..... = 25
1st response to tetan., m. −, br. +, = +0·0120 volt.
2nd ,, ,, m. +, br. −, = +0·0092 ,,
3rd ,, ,, m. −, br. +, = +0·0100 ,,
4th ,, ,, m. +, br. −, = +0·0076 ,,
Conductivity at end ..... = 35

Skin boiled.—Conductivity ..... = 115
1st response to tetan., m. −, br. +, = +0·0004 volt.
2nd ,, ,, m. +, br. −, = −0·0004 ,,
3rd ,, ,, m. −, br. +, = +0·0004 ,,
4th ,, ,, m. +, br. −, = −0·0004 ,,
Conductivity ..... = 115

(3.) The augmented conductivity of the boiled as compared with the living skin—from 35 to 115.

(4.) The unaltered conductivity of the boiled skin—115 before and after tetanisation.

The small deflections seen in the case of the dead skin are such as may be observed with a non-living electrolyte, and are due to polarisation. They follow the direction of the break current, and are due to the fact that the sum of polarisation countercurrents by the series of make currents is greater than the sum of similar effects by break currents.

The four deflections seen in the case of the living skin, do not vary in direction with varying direction of the exciting currents, but exhibit an inequality such that the + (outgoing) deflection after $\left\{\begin{matrix}\text{make} & - \\ \text{break} & +\end{matrix}\right\}$ is greater than the + deflection after $\left\{\begin{matrix}\text{make} & + \\ \text{break} & -\end{matrix}\right\}$. This inequality is such as would be produced by the polarisation effect witnessed alone in the dead skin. The four deflections of living skin exhibit a progressive decline, attributable to fatigue.

A similar inequality, attributable to polarisation (but possibly in part due to unequal excitation by the two poles of an exciting current) is witnessed in the outgoing effects of single break induction currents. The + (outgoing) response after an induction current of − direction is greater than the + response after an induction current of + direction. (Fig. 2.)

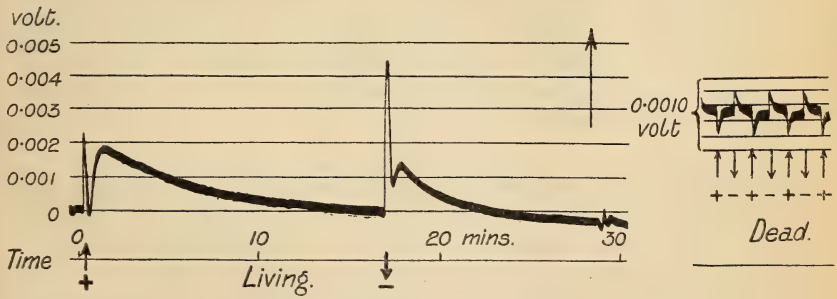


Fig. 2 (4201).—Skin of Breast 8 hours after amputation. *Living.*—Two + responses to single break induction shocks in + and − directions. 8 L. 10,000. *Dead.*—Several − and + effects to + and − shocks, *i.e.*, polarisation. Resistance diminished.

Effect of Previous Excitation.—The response to a particular excitation is greatly influenced by the previous excitation to which the skin may have been subjected. In general, the second of two moderate excitations produces a greater effect than the first, while the second of two strong excitations produces a smaller effect than the first. In the case

of living skin, the influence of single shocks (and still more of alternating currents) is considerable, and must be reckoned with in any estimate of voltage. In, *e.g.*, skin No. VII (*vide* Tabular Summary), the difference between the responses in the two directions of tetanisation at 10,000 units on the second and third days depends on the fact that the order of tests was reversed on the two days. For this reason, in any comparison between the effects of excitation in opposite directions one pair of trials is insufficient: two or more pairs of trials are necessary. In fig. 1, *e.g.*, the result of two pairs of trials is given. In fig. 4204 the decline of voltage is, in reality, much greater than is at first sight apparent, by reason of the augmented conductivity aroused

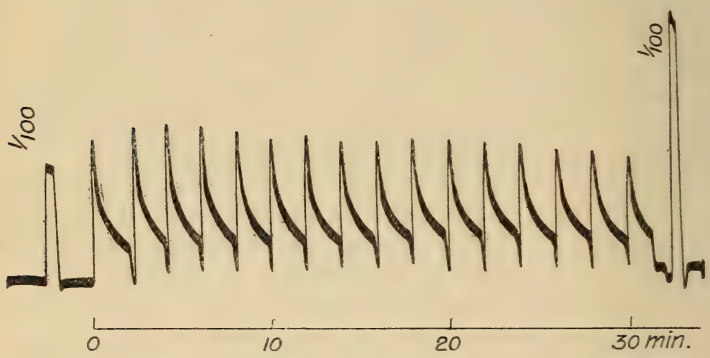


Fig. 3 (4204).—Same skin 22 hours after excision. Series of 16 positive responses to tetanisation for 8 seconds at intervals of 2 minutes. Coil at 1000, supplied by 8 L. Make —, Break +. Gradual decline of E.M.F.; which decline is greater than apparent by reason of increasing conductivity; the latter, as shown by the standard deflections at beginning and end of experiment, has been more than doubled.

by the succession of excitations. For the same reason it is not easy to make satisfactory observations of the variation in strength of response with variation in strength of excitation, even when the mean of successive effects is taken on ascending and descending scales, the influence of fatigue remains obvious, especially if (as ought to be done) the gradual increase of conductivity is taken into account.

For example :—

Skin No. VII. Second Day.

Strength of tetanisation. {make −. break +.}	Deflection.	Conductivity. (Deflection by 0·01 volt.)	Voltage of response.
1000	− 7	50	−0·0014
2000	+ 30	70	+0·0043
4000	+170	130	+0·0130
6000	+280	170	+0·0165
8000	+300	200	+0·0150
10000	+280	230	+0·0122
10000	+240	240	+0·0100
8000	+200	230	+0·0087
6000	+130	220	+0·0059
4000	+ 80	220	+0·0027
2000	+ 30	200	+0·0015
1000	+ 6	180	+0·0003
From which the mean values come out—			
1000	..	..	−0·0005
2000	..	..	+0·0024
4000	..	..	+0·0078
6000	..	..	+0·0012
8000	..	..	+0·0000
10000	..	..	+0·0111

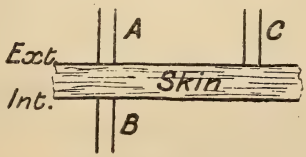
As far as my observations have gone, decline by fatigue has exhibited itself more prominently in the human skin than in the frog's skin. On the other hand, summation of effects has been much less apparent.

Locality of the Reaction.

The blaze currents of human skin arise exclusively from its external epithelial strata, not the most superficial keratinised cells, but the underlying and presumably living cells of the Malpighian layer.

This principal fact is demonstrated in various ways :—

(1.) A piece of fresh skin, put up as figured, is excited through the electrodes A, B, and led off to the galvanometer through A, C, and then through B, C (which are previously compensated before each trial).



There occur in consequence of excitation from A to B and from B to A—

response from C to A no response from C to B,

i.e., the electromotive spot has in each case been situated at or near the external surface A.

(2.) The subcutaneous tissue, and the corium itself, give no response. A slice taken from the sole of the foot and limited to the horny layer gives no response. A slice of skin taken so as to include the Malpighian epithelium gives more or less well-marked response.

And in this last connection I think it worthy of remark that the response is well marked in specimens of skin that did not include any distinct glandular masses. In the pads of the cat's foot *e.g.*, where I had expected to have to reckon with glandular as well as cutaneous epithelium, it was evident on microscopic examination that no glandular tissue was present, but only some scattered ducts belonging to deep-seated glands. The principal active part was clearly the deeply stainable layer of Malpighian epithelium.

The Reaction is Local.—The blaze reaction of the skin is exclusively local, and is not propagated to a distance from the excited spot. And as regards physiological state, it generally happens that closely adjacent portions may exhibit very different degrees of vitality. This has been particularly apparent in the case of mangled skin and of skin involved in cancerous growth. In both these cases it was not difficult to distinguish correctly between skin likely to give a poor reaction and skin likely to give a good reaction. But by reason of such local differences it is not easy to obtain satisfactory instances of declining vitality with lapse of time by taking the reaction of a series of different bits of the same piece of skin; in general, a decline was evident, but with frequent exceptionally low values at an early stage and exceptionally high values at a late stage—*e.g.*, No. VI on the sixth day gave +0·0100 and +0·0150 volt to single shocks—an exceptionally high value at this period.

Nevertheless, it is possible with care to distinguish amid such irregularities the general decline of skin vitality with lapse of time and the individual differences of vitality in different skins of men and animals.

As regards animals, I have, however, made as yet only occasional and unsystematic observations upon cats and rabbits, from which I have provisionally concluded that the skin of man is more enduring than that of either cats or rabbits; that the skin of cats is more resistant than that of rabbits, and that the skins of individual cats and rabbits exhibit considerable differences of endurance according to the state of nutrition at the moment of somatic death.

I have been much astonished, and am still somewhat incredulous, of

my own conclusion with regard to the endurance of human skin. On a healthy skin I have obtained what I take to be signs of life as long as 10 days after excision, and this did not appear to be a longest possible period. I was fully alive to possibilities of fallacy, and expended much care and time upon their experimental exclusion. Whether I have succeeded or not is matter for future investigation. Pathological observations on the human skin in relation to its surgical transplantation to some extent bear out the view that it may survive excision for an extraordinary length of time, especially in a semi-desiccated state and under antiseptic protection. Wentscher* found, *e.g.*, that skin grafts, preserved for 7 to 14 (and in one instance for 21) days, recovered vitality in 3 or 4 days, as indicated particularly by the reappearance of karyokinetic figures.

As regards skin taken from the *post-mortem* room, Schede (quoted by Wentscher) made successful transplantation of skin 12 hours *post-mortem*, but was unsuccessful with 24 hours' old material.

The conductivity of human skin, more especially of fresh human skin, is greatly augmented in consequence of electrical excitation. It is further increased by boiling, and the change occurring gradually in kept skin is of itself sufficient to greatly reduce the original resistance.

Thus, *e.g.*, in skin No. II, examined when I had not yet realised the great influence of previous electrical excitation, and did not therefore measure the resistance of the perfectly fresh skin, the conductivity is increased threefold in consequence of tetanisation, and tenfold in consequence of subsequent boiling.

In a later experiment directed specially to this point, the resistance measured by Wheatstone bridge was originally above 230,000 ohms, falling to below 100,000 ohms in consequence of strong tetanisation.

In my first observations, hardly anticipating any such considerable alterations of resistance, I contented myself with recording a standard deflection of 0·01 volt through the skin (+ electrodes + galvanometer) to see whether or no the resistance was appreciably altered during observation. But I also frequently recorded, for the sake of comparison, the standard deflection of 0·01 volt through a megohm (+ galvanometer) and was therefore able to utilise for the study of alterations of conductivity a considerable body of data directed to other questions. The alterations were indeed so considerable that it was possible without gross error to calculate absolute values of resistance (or conductivity) from the standard deflections recorded at beginning and end of observation. The comparison of such data with data obtained by direct measurement showed that the calculation was permissible. Of course the calculated data are valid only in the case of

* Ziegler's 'Beiträge zur Pathologischen Anatomie und zur Allgemeinen Pathologie,' vol. 24, p. 101, 1898.

the high resistances of unboiled skin, not in that of the low resistances of boiled skin, which, however, are of secondary interest.

The following instance will serve to illustrate the above mode of calculation :—

Skin No. II. 5 hours after excision. Galvanometer + electrode resistance = 20,000 w. Two or three preliminary tests made.

	Defl. by 1/100 volt.	Resistance.
At outset	5	540,000
After four periods of tetanisation	25	92,000
After four further periods (Plate 4199)	35	60,000
After boiling	115	? 4,500
After four further periods (Plate 4200)	115	? 4,500

(Defl. by 1/10 volt through 1 megohm = 26.)

and is (with other data) sufficient proof of the statement that fresh skin has a resistance to be measured in hundreds of thousands of ohms, tetanised skin a resistance of tens of thousands, boiled skin a resistance of thousands.

With regard to the cause of this remarkable augmentation of conductivity, we are in presence of two possible alternatives: 1st, a "kataphoric" migration of water; and 2nd, a dissociation of electrolytes.

I do not at present see my way to the sharp discrimination of these two possible factors, and can only bring forward considerations that appear to me to show that the second factor—electrolytic dissociation—is a chief cause of the increased conductivity, although no doubt transport of fluid, and indeed in certain cases rise of temperature may contribute to the effect.

The diminution of resistance is produced in far more marked degree in living skin than in dead skin. It is best produced in consequence of the summated effect of alternating currents; it is evident in consequence of a single strong induction shock. Thus, *e.g.* (Skin IV) an initial resistance of $1\frac{2}{3}$ megohm, was lowered to 1 megohm after two single induction shocks at 10,000, and to $\frac{1}{2}$ megohm after tetanisation at 1000 for two periods of 5 seconds.

Kataphoric alterations of resistance, as described by du Bois-Reymond, are in the sense of an augmentation caused by desiccation at the anode of a strong prolonged galvanic current. And although there can be no doubt that such anodic augmentation has as its counterpart a kathodic diminution of resistance by reason of augmented moisture, it is not *à priori* very probable that the great alteration of skin resistance caused by one or several induction shocks is due to predominant kathodic augmentation of moisture.

On the other hand, I have observed a case, that of the hen's egg, in which induction shocks give a similar considerable increase of conductivity which I find it difficult to understand otherwise than as an effect of water transport from electrode to shell at the anodic side, and from contents to shell at the kathodic side.

I am constrained therefore to leave undetermined the possible influence of kataphoric action on skin resistance until I shall have found means of investigating the phenomenon further.

Alterations of temperature produce alterations of resistance of the skin as of any moist conductor, viz., augmentation of resistance with lowered temperature, and diminution of resistance with raised temperature. In the case of the *living* skin (as in that of some other living tissues) I have witnessed at the moment of congelation and on subsequent thawing, two well-marked effects that appear to be most significant of a phenomenon of dissociation. At a critical temperature ($-4°$ to $-6°$ of the cooling chamber) a sudden electromotive discharge takes place, attributable to the sudden excitation or explosion of living matter in the act of congelation. Subsequently, on return of the frozen skin to the original temperature, the resistance is very much reduced, a change which is attributable to mechanical or chemical dissociation of the previously frozen tissue-elements.

Thus, *e.g.*, in Experiment 4209 the resistance of the skin (corrected for electrode resistance) at 18° was 150,000 ohms before congelation,

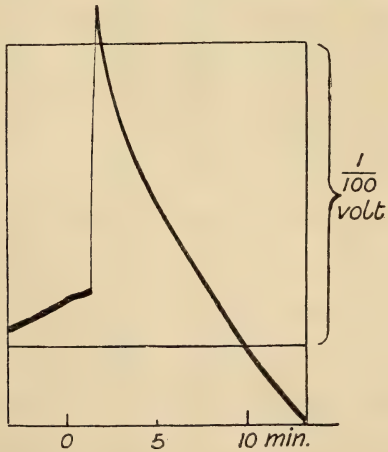


FIG. 4 (4209).—Skin No. III, 2nd day after excision. Skin gradually cooled by surrounding the skin-chamber with a freezing mixture. Sudden electromotive discharge (outgoing current) at a temperature of $-6°$ inside the skin-chamber. Before freezing, the + responses to + and − single induction shocks were +0·004 and +0·008 volt. After freezing, the + responses were absent, being replaced by small − and + polarisation effects. On recongelation no second discharge was observed.

and after congelation 50,000 ohms at 14°. At the temperature of −6° of the cooling chamber a sudden electromotive discharge of +0·0080 volt took place. The responses to excitation of +0·0040 and +0·0080 previous to congelation, were abolished in consequence of congelation.

Sources of Fallacy—Doubtful Cases.

In ordinary cases (*i.e.*, with healthy skin a few hours after removal from the body) and with ordinary care, the results of experiment are unmistakable, and there is no room for doubt. A clear, positive response of more than 0·01 volt, to single shocks of both directions and to alternating currents of both pairs of directions, is proof that the skin is living.

But at later periods, and in cases of skin obtained from the *post-*

Voltage of Response of Human Skin after Excitation by Induced

	Plate No.	Time.	Single break current.		Tetanisation for 5 seconds by			
			10000.		1000.		5000.	
			+ (outgoing).	− (ingoing).	{make − break +}	{make + break −}	{make − break +}	{make + break −}
No. I. Skin of amputated leg	4187	3 hrs.	+0·0005	+0·0010	—	—	—	—
	4188	4 ,,	—	—	—	—	—	—
	4189	—	—	—	—	—	—	—
	4190	6 hrs.	+0·0020	+0·0025	+0·0030	—	—	—
	4192	2nd day	+0·0005	+0·0010	+0·0020	+0·0010	—	—
	4193	3rd ,,	−0·0002	+0·0004	+0·0010	nil	—	—
	4194	,, ,,	−0·0001	+0·0001	—	—	—	—
	4195	4th ,,	+0·0005	+0·0010	+0·0010	+0·0020	—	—
	,,	,, ,,	−0·0001	+0·0001	—	—	+0·0003	−0·0005
	4196	5th ,,	—	—	—	—	+0·0005	+0·0006
	,,	,, ,,	−0·0001	+0·0001	—	—	—	—
	4197	9th ,,	−0·0001	+0·0002	—	—	+0·0020	−0·0010
	,,	,, ,,	−trace	+trace	—	—	—	—
No. II. Skin of amputated breast.	4197*a*	1½ hrs.	+0·0180	+0·0230	+0·0440	+0·0460	—	—
	,,	,,	—	—	—	—	—	—
	4198	3 ,,	—	—	+0·0040	+0·0080	+0·0300	+0·0200
	4199	5 ,,	—	—	—	—	+0·0120	+0·0092
	4200	5¾ ,,	—	—	—	—	+0·0004	−0·0004
	4201	8 ,,	+0·0022	+0·0044	—	—	+0·0100	+0·0090
	,,	,,	—	—	—	—	—	—
	4203	21 ,,	+0·0050	+0·0175	+0·0100	+0·0250	—	—
	4204	22 ,,	—	—	+0·0126	—	—	—
	4205	23 ,,	+0·0040	+0·0060	—	—	—	—
	4206	3rd day	−0·0010	+0·0005	—	—	+0·0100	+0·0150
	4207	4th ,,	+0·0025	+0·0035	—	—	—	—
	,,	,,	−trace	+trace	—	—	—	—
	—	5th ,,	—	—	—	—	+0·0100	+0·0010
	4213	7th ,,	+0·0040	+0·0050	+0·0050	+0·0060	—	—
	—	8th ,,	−0·0005	+0·0002	—	—	+0·0013	−0·0011
No. III. Skin of abdomen.	—	6 hrs.	—	—	+0·0012	+0·0009	—	—
	4208–9	2nd day	+0·0040	+0·0080	—	—	—	—
	,,	,,	−trace	+trace	—	—	—	—
	4210	3rd day	+0·0011	+0·0021	+0·0007	+0·0040	—	—
	4212	4th ,,	+0·0023	+0·0058	—	—	—	—

mortem room, more or less doubtful results may be obtained, and unless due regard is paid to possible sources of fallacy, wrong conclusions may be drawn.

A principal source of fallacy lies in the electrodes; it is less liable to be of disturbing effect in the simple case of excitation by single shocks; it is more liable to give rise to ambiguous results in the more doubtful cases where alternating currents are required, the resulting polarisation in such case being less easy to recognise as such than in the case of single shocks. In such cases, when single shocks produce only polarisation effects, the voltage of any true response that may co-exist is low, and although alternating currents may then bring it into evidence, still we are not wholly free from a polarisation resultant which is of complicated origin and of inconstant direction.

Currents of a Berne Coil, supplied by 8 Leclanché Cells (11·5 volts).

alternating currents. 10000.		Conductivity. (Not corrected for galvanometer and electrode resistance.)	Resistance.	
{make + / break −}	{make + / break −}			
>+0·0100	>+0·0100	60	166667	Ingoing effects −0·0020 and −0·0015 with weak tetan.
+0·0350	+0·0180	75	133333	Another piece.
+0·0015	−0·0002	300	33333	Same piece, boiled.
+0·0500	+0·0200	30	333333	Another piece.
—	—	110	90909	Another piece.
—	—	210	47619	Another piece. Doubtful state.
+0·0006	−0·0006	210	47619	Same piece, boiled.
—	—	70	142857	Another piece.
—	—	200	50000	Ditto, boiled.
+0·0007	+0·0005	210	47619	Another piece. Doubtful.
—	—	210	47619	Ditto, boiled.
—	—	150	66667	Another piece. Doubtful.
+small	−small	250	40000	Ditto, boiled.
—	—	10	1000000	Good response. Cond. × 2 by tetanisation.
+0·0005	−0·0005	200	50000	Same piece, boiled. Cond. × 10 by boiling.
—	—	5	560000	Another piece. Galv. shunted 1/3. Cond. × 5.
—	—	25	112000	Same piece. Cond. × 7.
—	—	115	24348	Ditto, boiled. Cond. raised to 115, and not further altered by tetanisation.
—	—	40	250000	Another piece. Galv. unshunted.
+0·0002	−0·0003	200	50000	Ditto, boiled.
—	—	7	500000	Another piece. Galv. shunted 1/3. Cond. × 2·5 by tetan.
—	—	15	186667	Same piece. Series at +1000. Cond. × 35/15 by tetan.
—	—	35	80000	Ditto, ditto.
—	—	20	140000	Ditto, ditto, half dry.
+0·0550	+0·0160	7	500000	Another piece, half dry.
+small	−small	90	31111	Ditto, boiled.
+0·0060	+0·0020	15	186667	Another piece. Galv. 1/3. Cond. × 85/15 by tetan.
—	—	36	277778	Skin of nipple. Galv. unshunted. Cond. × 44/36. The surrounding skin is inactive.
—	—	180	55555	Ditto, boiled.
>+0·0100	>+0·0100	70	142857	Normal current = −0·0050.
—	—	60	166667	Another piece. E.M. discharge at −6°.
+small	−small	150	66667	Same piece at +14° after ½ hour at −6°.
—	—	80	125000	Another piece.
+0·0160	—	70	142857	Another piece.

Voltage of Response of Human Skin after Excitation by Induced

	Plate No.	Time.	Single break current.		Tetanisation for 5 seconds by			
			10000.		1000.		5000.	
			+ (outgoing).	− (ingoing).	{make − break +}	{make + break −}	{make − break +}	{make + break −}
No. IV. Skin of amputated leg.	—	3 hrs.	+0·0114	+0·0157	+0·0110	+0·0270	—	—
	—	4th day	+0·0063	+0·0043	—	—	+0·0166	+0·0166
	—	5th ,,	—	—	—	—	+0·0050	+0·0030
	—	6th ,,	+0·0001	+0·0004	+0·0002	+0·0012	+0·0064	+0·0042
	—	7th ,,	—	—	−0·0004	+0·0004	+0·0025	+0·0007
	—	8th ,,	−trace	+trace	{−0·0002 +0·0002}	−0·0003	—	—
	—	9th ,,	−0·0002	+0·0005	+0·0016	+0·0016	+0·0058	+0·0006
	—	10th ,,	+0·0005	+0·0005	—	—	+0·0090	+0·0110
	—	11th ,,	+0·0005	+0·0002	+0·0004	+0·0002	+0·0040	+0·0016
No. V. Skin of thigh from the *post-mortem* room. Death by gastric carcinoma.	—	48 hrs.	—	—	—	—	—	—
	—	,,	—	—	—	—	—	—
No. VI. Skin of amputated breast.	—	2nd day	+0·0100	+0·0100	+0·0050	+0·0150	+0·0067	+0·0060
	—	3rd ,,	+0·0040	+0·0054	+0·0042	+0·0125	+0·0350	+0·0100
	—	4th ,,	−0·0006	+0·0001	−0·0002	+0·0002	+0·0003	+0·0010
	—	5th ,,	+0·0001	+0·0002	nil	nil	+0·0004	+0·0006
	—	6th ,,	+0·0060	+0·0060	nil	+0·0005	+0·0074	+0·0024
	—	,,	nil	+0·0002	—	—	—	—
	—	,,	nil	nil	—	—	—	—
	—	7th ,,	—	—	—	—	—	—
	—	,,	+0·0001	+0·0001	+0·0001	−trace	+0·0011	−0·0003
	—	,,	+0·0001	+0·0001	+0·0022	+0·0010	+0·0081	+0·0012
	—	,,	+0·0006	+0·0009	+0·0003	nil	+0·0017	nil
	—	,,	nil	nil	nil	nil	+0·0003	−0·0002
	—	8th ,,	nil	+trace	—	—	—	—
No. VII. Skin of amputated leg.	—	1st day	−0·0005	+0·0014	−0·0016	+0·0009	+0·0080	+0·0014
	—	,,	+0·0020	+0·0020	+0·0010	+0·0005	+0·0113	+0·0026
	—	2nd ,,	−0·0010	−0·0005	−0·0010	+0·0012	+0·0105	+0·0018
	—	3rd ,,	+0·0004	+0·0004	+0·0003	+0·0062	+0·0016	+0·0006
	—	4th ,,	−trace	+0·0006	+0·0012	−0·0005	+0·0050	−0·0025
	—	,,	−trace	+trace	—	—	—	—
	—	5th ,,	nil	+0·0003	{+0·0003 −0·0006}	−0·0004	−0·0030	+0·0040
	—	,,	{−0·0002 +0·0002}	+0·0010	+0·0025	−0·0025	+0·0075	−0·0033
	—	,,	−0·0001	+0·0002	+0·0008	+0·0005	+0·0026	−0·0003
	—	,,	—	—	—	—	—	—
	—	6th ,,	−0·0003	+0·0006	+0·0002	+0·0008	+0·0011	+0·0025
	—	,,	−trace	+trace	+0·0002	−0·0005	+0·0005	−0·0010
	—	,,	−0·0001	+trace	+0·0001	−0·0003	+0·0009	−0·0016
No. VIII. Skin from *post-mortem* room. Sudden death from heart disease.	—	3rd day	+0·0010	+0·0010	+0·0010	−0·0010	+0·0165	−0·0045
	4216	,,	+0·0010	+0·0010	+0·0010	+0·0010	+0·0080	+0·0005
	—	,,	+0·0005	+0·0005	—	—	—	−0·0012
	—	,,	−trace	+trace	—	—	—	—
	—	4th ,,	+0·0002	+0·0006	+0·0012	+0·0005	+0·0115	+0·0015

Currents of a Berne Coil, supplied by 8 Leclanché Cells (11·5 volts)—*continued.*

alternating currents. 10000.		Conductivity.	Resistance.	
make + / break −	make + / break −	(Not corrected for galvanometer and electrode resistance.)		
—	—	6, 10, 20	1666667 1000000 500000	Normal current = −0·0180. Conductivity increasing by single shocks and tetan.
—	—	40	250000	Normal current = +0·0100.
+0·0120	+0·0010	10, 40	1000000 250000	
+0·0087	+0·0018	200	50000	
+0·0075	+0·0012	50, 80	200000 125000	
+0·0100	−0·0080	20, 30	500000 333333	
+0·0025	−0·0005 +0·0005	75, 160	133333 62500	
+0·0050	+0·0090	28, 62	357143 161300	
+0·0035	+0·0030	125, 300	80000 33333	Fat removed. Normal current = +0·0060.
+0·0050	+0·0040	400	25000	
+0·0005	−0·0005	800	12500	Boiled.
+0·0072	+0·0060	28, 50	357143 200000	Normal current = −0·0140.
+0·0162	+0·0042	40	250000	
+0·0016	+0·0020	400	25000	Cancerous tissue gives no response.
+0·0008	+0·0012	500	20000	
+0·0250	+0·0150	15, 90	666667 111111	Good response.
+0·0030	−0·0001	300	33333	Same piece, boiled.
+0·0004	−0·0001	750	13333	Electrodes alone.
+0·0020	−0·0003	450	22222	New electrodes ; same piece of boiled skin.
+0·0025	+0·0002	300, 300	33333	Skin of nipple.
+0·0168	+0·0038	150	66667	Adjacent piece.
+0·0090	+0·0010	160	—	Ditto, fat removed.
+0·0005	−0·0004	550	—	Ditto, boiled.
+0·0020	−0·0010	125	80000	Skin dead ; putrefaction apparent.
—	—	90	111111	An injured piece of skin.
+0·0116	+0·0020	300	33333	Another and better piece.
+0·0100	+0·0025	50, 180	200000 55556	
+0·0032	+0·0073	150, 200	66667 50000	
+0·0100	−0·0060	100	100000	Another piece ; doubtful state.
+0·0017	−0·0024	400	25000	Ditto, boiled.
−0·0050	+0·0100	75, 100	133333 100000	Another piece ; anomalous response.
+0·0064	−0·0030	50, 75	200000 133333	Ditto, an hour later.
+0·0025	−0·0008	200	50000	Ditto, boiled.
+0·0004	−0·0008	400	25000	The electrodes alone.
+0·0010	−0·0002 +0·0005	100	100000	Same piece of skin replaced on same electrodes.
—	—	400	25000	The electrodes alone.
+0·0008	−0·0020	150	66667	Same piece of skin on fresh electrodes.
+0·0300	−0·0040	20, 50	500000 200000	
+0·0100	−0·0025	40, 80	250000	
—	−0·0010	80, 120	125000	
—	—	120	83000	After boiling.
+0·0100	+0·0040	40	250000	

Voltage of Response of Human Skin after Excitation by Induced

	Plate No.	Time.	Single break current.		Tetanisation for 5 seconds by			
			10000.		1000.		5000.	
			+ (outgoing).	− (ingoing).	{make − / break +}	{make + / break −}	{make − / break +}	{make + / break −}
No. IX. Skin from *post-mortem* room. Death by heart disease.	—	3rd day	+0·0002	+0·0003	+nil	nil	+0·0025	+0·0003
	—	4th ,,	nil	+0·0004	+trace	−trace	+0·0030	−0·0012
	—	5th ,,	+0·0040	+0·0080	nil	nil	+0·0090	−0·0005
	—	,, ,,	−0·0001	+0·0001	+0·0001	−0·0001	+0·0008	−0·0004
	—	6th ,,	−0·0003	+0·0003	+0·0008	—	+0·0070	−0·0020
	—	,, ,,	−0·0001	+0·0001	—	—	—	—
	—	12th ,,	−0·0002	+0·0006	—	—	—	—
No. X. Skin of amputated arm.	—	1st day	+0·0012	+0·0050	—	—	—	—
	—	6th ,,	−0·0008	+0·0016	—	—	—	—
			−trace	+0·0005	—	—	—	—
No. XI.	—	1st day	—	—	+0·0003	+0·0002	+0·0044	+0·0012
	—	2nd ,,	—	—	+0·0001	nil	+0·0030	−0·0020
	—	3rd ,,	—	—	+0·0017	+0·0067	−0·0050	{−0·0017 / +0·0013}
	—	4th ,,	−0·0010	−0·0007	−0·0045	−0·0018	−0·0110	−0·0087
	—	5th ,,	−0·0002	+0·0002	+0·0003	−0·0004	+0·0010	−0·0022
	—	6th ,,	+0·0003	−0·0002	+0·0002	−0·0005	+0·0014	−0·0015
No. XII. Great Ormond St. Foreskin, infant, of March 4th, 1902.	—	2nd day	+0·0050	+0·0025	+0·0250	+0·0200	—	—
	—	32 hrs.	—	—	+0·0110	+0·0100	+0·0159	+0·0133
	—	3rd day	+0·0006	+0·0022	+0·0020	+0·0011	+0·0076	+0·0031
	−0·0020	4th ,,	−0·0017	+0·0002	+0·0010	−0·0005	+0·0037	−0·0042
	−0·0006	5th ,,	−0·0002	+0·0002	+0·0004	−0·0004	+0·0022	−0·0022
No. XIII. March 4th was 1st day.	−0·0030	2nd day	+0·0010	+0·0014	+0·0015	+0·0012	+0·0045	+0·0036
	—	3rd ,,	+0	—	+0·0015	+0·0012	+0·0040	+0·0033
No. XIV. Guy's Hospital. Foreskin, child.	−0·0012	2nd day	+0·0003	nil	+0·0006	+0·0001	+0·0006	+0·0023
	−0·0034	3rd ,,	nil	+0·0007	+0·0013	−0·0007	+0·0117	+0·0008
	—	4th ,,	−0·0006	+0·0005	+0·0002	−0·0002	+0·0030	−0·0016
	−0·0002	5th ,,	−0·0004	+0·0003	+0·0001	−0·0001	+0·0020	−0·0017
	—	6th ,,	−0·0006	+0·0006	nil	nil	+0·0050	−0·0020
No. XV. Great Ormond St. Foreskin, of March 4.	−0·0013	3rd day	+0·0005	+0·0020	+0·0016	−0·0004	+0·0070	+0·0027
	—	4th ,,	+0·0009	+0·0011	+0·0004	−0·0001	+0·0030	−0·0007
	—	5th ,,	+0·0003	+0·0003	+0·0012	−0·0004	+0·0025	+0·0020
No. XVI. Ditto. ...	−0·0030	4th day	−0·0006	+0·0004	nil	nil	+0·0060	+0·0032
	−0·0010	5th ,,	−0·0002	+0·0007	,,	,,	+0·0016	{+0·0002 / −0·0002}
	—	6th ,,	−0·0030	+0·0030	,,	,,	+0·0028	−0·0032
No. XVII. Ditto. ...	−0·0030	6th day	+0·0002	+0·0005	—	—	—	−0·0033
No. XVIII. Great Ormond St. Foreskin of March 11th.	−0·0061	2nd day	+0·0007	+0·0021	+0·0008	+0·0005	+0·0034	+0·0015
	—	3rd ,,	+0·0008	+0·0037	+0·0026	+0·0007	+0·0073	+0·0023

Currents of a Berne Coil, supplied by 8 Leclanché Cells (11·5 volts)—*continued.*

alternating currents. 10000. {make − break +}	{make + break −}	Conductivity.	Resistance.	
+0·0050	+0·0005	130		
+0·0050	−0·0006	100	60000	After tet. 47000. R. measured.
+0·0300	−0·0035		1100000	Then 214000.
+0·0012	−0·0004			After boiling, only polarisation.
+0·0095	−0·0024			Another piece.
+0·0016	−0·0008			Ditto, boiled.
+0·0016	−0·0003	250, 250		R. unaltered by tet.
+0·0114	+0·0014	8, 14		In this observation the fallacy of the electrodes became obvious.
+0·0080	−0·0020	50, 90		
+>0·0100	+>0·0100	240		Skin boiled.
+>0·0100	+>0·0100	—		Electrodes alone.
+trace	−trace	—		Electrodes reamalgamated.
+trace	−trace	—		Skin replaced.
+0·0070	+0·0016		2000000	Reduced to 200000 W after tet.
+0·0060	+0·0030			
−0·0037	−0·0055			
−0·0060	−0·0133			
+0·0006	−0·0025			
+0·0017	−0·0029			
—	+0·0070	0·01=20		4 Lecl.
—	—	0·01=44		2 ,,
+0·0058	+0·0021	0·001= 5		,, ,,
+0·0060	−0·0050	0·001= 4		,, ,,
+0·0016	−0·0030	0·001=10		,, ,,
+0·0045	+0·0030	—	600000	Ohms.
+0·0038	+0·0025			
+0·0008	+0·0031	0·001=10	—	4 Lecl.
+0·0175	+0·0053	,, 6	—	2 ,,
+0·0036	−0·0042	,, 4	—	,, ,,
+0·0030	−0·0030	,, 7		
+0·0080	−0·0023	,, 3		
+0·0052	+0·0021	0·001=10		
+0·0062	+0·0008	0·001=11		
+0·0038	+0·0016	0·001=11		
+0·0120	+0·0044	0·001 5		
+0·0040	+0·0005	0·001 10		
+0·0071	−0·0017	0·01 6		
+0·0070	−0·0037	0·001= 4		
+0·0066	+0·0044	0·001= 9		
+0·0128	+0·0036	0·001=13		

General Remarks concerning the Preceding Table.

The table comprises groups of observations. Nos. I to XI were taken with skins sent to me by Mr. Plimmer from the operating theatre and the *post-mortem* room of St. Mary's Hospital. Nos. XII to XVIII were taken with, for the most part, an amputation skin sent to me from Guy's Hospital and from Great Ormond Street by the kind directions of Mr. Lane.

The data of both groups are in complete agreement with the statements made in the text, and with the representative experiment of which the graphic record is reproduced in fig. 1, viz., to both directions of single induction shocks, and to both pairs of directions of alternating tetanising currents the electrical response of living skin was outgoing (+).

A maximal value of the response under favourable conditions was + 0·04 to + 0·05 volt.

It was noticed on more than one occasion that the summated effect of tetanisation was of about equal voltage to the single response, to a single induction shock ten times as strong.

It was noticed more than once that the electrical response was small immediately after excision, greater 24 hours later, and subsequently diminishing day by day. I consider it probable that the smallness of the response of quite freshly excised skin is an effect of the excitation or "shock" of manipulation. In one particular instance, in which the skin had been very thoroughly cleared of subcutaneous tissue, I observed little or no response on the 1st day, and a typical response on the 2nd and succeeding days.

Skin No. 2 observation was most carefully followed out. It was undoubtedly living on the 7th day, when the following record was obtained, which illustrates

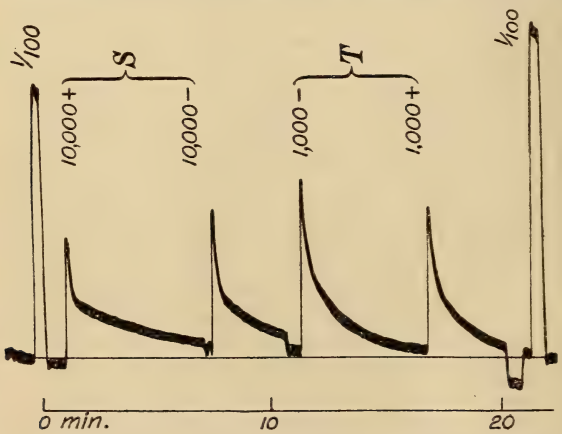


FIG. 5 (4213).—Skin No. 2. Close to nipple. 7th day after excision. Positive (outgoing) responses to + and − break induction currents (coil at 10,000), and to brief tetanisation in both pairs of directions (coil at 1000).

at the same time the fact mentioned above, viz., that the summated effects at a given strength of excitation are approximately equal to the single effects at ten times that strength.

The same skin afforded data that illustrate the progressively declining "vitality" of surviving skin. The following tabular summary of notes will suffice to illustrate this point:—

	Exc. by break 10,000 +.	Exc. by break 10,000 −.
1st day	+ 0·0180 volt	+ 0·0230 volt
2nd „	+ 0·0050 „	+ 0·0175 „
3rd „	—	—
4th „	+ 0·0020 „	+ 0·0030 „
5th „	—	—
6th „	—	—
7th „	+ 0·0040 „	+ 0·0047 „ (skin of nipple, fig. 5)
8th „	− 0·0005 „	+ 0·0005 „ (ordinary skin, polarisation only).

In all cases where note was taken of the "normal current" this was found to be of ingoing direction, *i.e.*, contrary to what might be given as current of injury from inner to outer surface. The voltage was in no case nearly as high as the voltage of a response—*e.g.*, the following are values taken almost at random from my laboratory note-book :—

Skin No.	I	1st day	−0·0027 volt.	
„	II	1st „	−0·0100 „	
„	„	2nd „	−0·0044 „	
„	„	3rd „	−0·0050 „	
„	„	4th „	−0·0040 „	
„	„	7th „	−0·0037 „	(nipple).
„	III	2nd „	−0·0040 „	
„	„	3rd „	−0·0060 „	
„	IV	1st „	−0·0130 „	

In the second group of cases, Nos. XII to XVIII, the skins were those of infants on whom circumcision had been performed, and included therefore a layer of mucous membrane. In these cases also the normal current as regards the layer of true skin was ingoing and the response was outgoing. But the results are complicated by reason of the mucosa, which as far as I have yet seen responds usually by an ingoing current. Further observations directed to this point are, however, necessary.

The conductivity and its modifications are in all cases indicated by means of standard deflections by a given voltage—usually 0·01 volt. The resistance, in a few instances wheatstoned, is generally calculated by reference of the deflection to that of the same voltage through a megohm—and usually the disposition of the galvanometer was such that 0·01 volt through 1 meghom (*i.e.* $1 \cdot 10^{-8}$ amp.) gave a deflection of 10 mm. The values for conductivity are given in millimetres of deflection by 0·01 volt, and each millimetre expresses therefore $1 \cdot 10^{-9}$ mho.

"On the Parasitism of *Pseudomonas destructans* (Potter)." By M. C. POTTER, M.A., F.L.S., Professor of Botany in the University of Durham College of Science, Newcastle-upon-Tyne. Communicated by Sir MICHAEL FOSTER, K.C.B., Sec. R.S. Received April 7,—Read June 12, 1902.

Since the publication of my paper upon a "Bacterial Disease of the Turnip,"* in which the existence of both a cytase and a toxin secreted by the bacterium was proved, I have pursued my investigations further, studying the action of the cytase and toxin upon the living cells, and have succeeded in tracing the passage of the bacterium into the cells through the cell-wall.

I think it will be sufficient for me to state that the strictest methods of sterilisation have been carefully observed throughout the work, and all sections of turnip were prepared as described in my former paper.

To observe the action of *P. destructans* upon a living cell, a small section of sound turnip was suspended in a hanging drop upon a Stricker's warm-stage, the lower opening of which was closed with a plate of glass cemented to the stage, a little water being introduced immediately before placing the cover-slip in position. The thermometer enabled me to tell the temperature of the preparation (which varied between 15° and 20° C.), and the two tubes leading into the central cavity supplied the requisite amount of air for the growth of the bacterium. Into the hanging drop a small fragment of turnip, inoculated with a pure culture of *P. destructans*, was introduced before the cover-slip was inverted over the Stricker's stage.

The effect of introducing the *Pseudomonas* was most striking and manifested a rapidity of action for which I was hardly prepared. The swelling of the wall could be recognised almost immediately, very soon the position of the middle lamella became visible as a much darker line, and contraction of the protoplasm quickly set in.

To take one particular case, a section was mounted at 10.30 A.M. and a cell selected for observation, which was uninjured by the razor and at the same time near the edge of the section. A wall common to this cell and the adjacent one was measured and found to be 2·5 μ in thickness; at 10.45 the bacteria were hovering round the wall; at 11.0 the wall measured 4·3 μ, and the track of the middle lamella was distinctly defined; at 11.20 the wall measured 6·5 μ; at 11.45 the two parts of the cell commenced to separate, and at 12 o'clock a gap of 2·5 μ separated the two walls.

The first signs of contraction of the protoplasm appeared at 11.15, and by 12 o'clock all the protoplasm had separated from the cell-wall

* 'Roy. Soc. Proc.,' vol. 67.

and formed an irregular bag in the centre of the cell. This contraction was not due to mere plasmolysis, but to the death of the cell under the action of the toxin, as the protoplasm never returned to its original position when placed in water.

Thus, within an hour and a-half of the introduction of the *Pseudomonas*, the cell was dead and its walls well advanced in a process of disintegration. After this the changes were less rapid, and, beyond a slight further separation of the cells, a more watery and rotten appearance of the cell-wall was all that could be observed.

Meanwhile the adjacent cells at the edge of the section were all being attacked in a precisely similar manner; contraction of the protoplasm set in at 11.30, and the decay could be observed gradually spreading inwards.

The original cell was kept under observation for some days and the development watched. The bacteria continued swarming around the cell-walls, and next morning (by which time the cells had been destroyed several layers deep) many bacteria had come to rest in contact with the wall, their long axis being perpendicular to its surface; and one or two had the appearance of being embedded in the wall as if in the act of boring their way through. I next attempted to watch a single individual in the hope of seeing it penetrate the wall; this at first seemed rather a hopeless task, but ultimately I was fortunate in fixing upon an individual which was just coming to rest. This bacterium was then kept continuously in view, and, after assuming a position perpendicular to the wall, it could be distinctly seen slowly forcing its way through until finally it emerged into the cell-cavity. The penetration of the wall was subsequently observed on several occasions, and numerous individuals could be seen in all stages of the process. The time required varied with the thickness of the wall, but on an average occupied about 3 hours. To give a special instance, a particular bacterium which was found to be just entering the wall at 11.30 A.M. emerged at 2.10 P.M. (fig. 1).

FIG. 1.—*Pseudomonas destructans* passing through a cell-wall. The bacterium was observed to enter the cell-wall at 11.30 A.M. (A) and to emerge into the cell at 2.10 P.M. (B). (Hanging drop; Zeitz obj. 7, oc. 4.)

The observation of the movements of the bacteria, though difficult and very trying, was yet considerably furthered by the different re-

frangibility of the cell-wall and the bacteria, this difference enabling the course of the bacteria to be distinctly followed.*

Important evidence that *P. destructans* has the power of perforating the cell-wall was also afforded by the well-known method of paraffin sections. Small sterile pieces of turnip were inoculated with a pure culture of *P. destructans*, and after 12 hours were found to be partially rotten. These were then fixed in Muller's and also in Flemming's fluid, washed, dehydrated, and embedded in paraffin. The sections were then cut, floated in water, and fixed to the slide by means of the white of egg. The paraffin was dissolved in turpentine. The slides were next placed in absolute alcohol, and then in gradually decreasing strengths of alcohol, for the purpose of staining. When fixed in Flemming's solution, it was found that the bacteria did not readily stain, and for this reason Muller's was preferred.

In staining the sections a further problem presented itself, namely, how to differentiate between the cell-wall and the bacterium. After numerous trials it was found effective to employ a weak aqueous solution of ruthenium-red, which was first allowed to act, the cell-walls being stained by this means but not the bacteria: the sections were then washed in water and stained with Ziehl's carbol-fuchsin or other aniline dyes. Subsequently ruthenium-red followed by Löwit's method for staining flagella was found to give the best results.

This method of fixing and double staining distinctly differentiated the cell-wall and bacteria, and showed the latter fixed in the actual process of perforating the wall, and various stages of penetration could be distinguished (fig. 2). These results confirm my observation of the penetration of the wall by *P. destructans*.

An organism which is thus capable of secreting a powerful cytase and toxin, producing such a remarkable effect in destroying living plant cells, and which subsequently has the power of perforating the cell-wall and entering the cell-cavity, must certainly be regarded as producing a true plant disease. Indeed the parasitic action of this bacterium upon living tissues is exactly comparable with that of certain of the parasitic fungi, though differences in detail are naturally presented from the different character of the organisms.

According to my previous observations, an attack of *P. destructans* could always be traced to a wound. I have found that this bacterium has no power to penetrate the cuticle of the mature epidermis. A number of blocks of turnip cut with the usual precautions, and to include a portion of the uninjured epidermis, were inoculated on the internal parenchyma, placed in sterile plugged test-tubes, and incubated at 30° C. for five days. At the conclusion of this period the pieces of

* It might be objected that the cells in the hanging drop were not under normal conditions, and thus their vitality might be impaired, but no such objection can be raised in the case of the cells of the blocks employed for the paraffin sections.

turnip were quite rotten, but the cuticle remained intact, and could be readily separated as a fine membrane. The experiment was also tried upon leaves and young shoots; in this case I employed no sterilising agent, which would have been liable to kill the very young epidermal cells. A small portion of turnip about the size of a pin's head, rotten through the action of *P. destructans*, was placed on the uninjured surface of a number of mature leaves, and also upon very young ones exposed by removing the outer leaves at the growing points, both sets being kept damp at the temperature of 30° C. After 5 days no sign of decay could be detected upon the mature leaves,

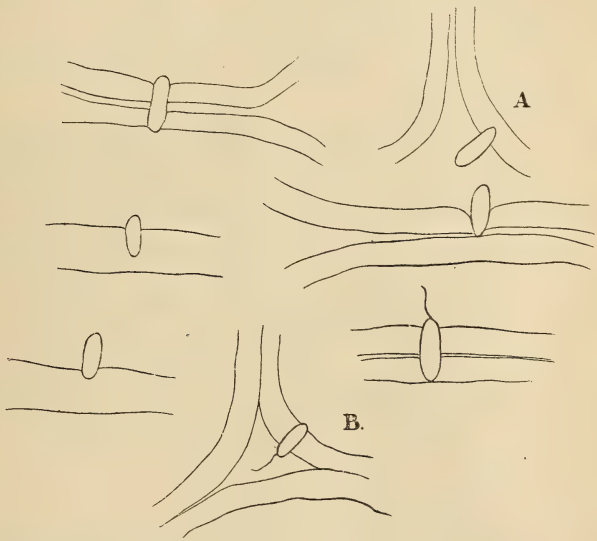


FIG. 2.—Stages in the perforation of the cell-wall by *Pseudomonas destructans*. At A and B the bacterium is seen passing into the cell from an intercellular space. (Paraffin sections; 1/12 oil immersion. Compensating oc. 8.)

but in the young ones the result was very different. These being in a growing condition, and possessing little or no cuticle, offered but a feeble resistance, and after the interval of 5 days had become quite rotten.

The old and fully developed cuticle is apparently proof against the action of the enzymes excreted by *P. destructans*, but this parasite can readily effect an entrance into its host through the undeveloped epidermis of young and tender structures.

This bacterium is incapable of manipulating the hard and tough rind of the sound turnip, but when brought in contact with a wounded surface it at once flourishes as a saprophyte upon the remains of the injured cells; very soon the number of individual bacteria becomes

largely increased, each one contributes its share of toxin and cytase, and in a very short time these products have sufficiently accumulated to kill the first cell. With the death of its protoplasm the cell-contents are liberated, and an additional supply of nutriment is thus provided;* the bacteria continue to multiply, cytase and toxin continue to be set free, and thus each cell succumbs in turn. It is not, however, until the protoplasm has been killed and the cell-wall very much softened that the bacteria have the power of perforating the walls and passing into the cell-cavity. It would hardly be supposed that a single bacterium, through its own excretions, could soften the wall and pierce it at one definite point after the manner of a fungus germ-tube. The extreme minuteness of the bacteria and the rapidity of their multiplication lead them to act, as it were, in concert, and the wall becomes softened by the cumulative action of many bacteria before the penetration of a single individual.

A comparison of the parasitism of *Botrytis cinerea* as demonstrated by the recent investigations of Nordhausen† presents an exact parallel. He has shown that the spore of this fungus excretes a powerful toxin in its initial stages of germination before any trace of the germ-tube can be detected. Its manner of effecting an entrance into a host-plant is first to kill the cell by the emission of the toxin; the germ-tube then penetrates the dead cell and is nourished saprophytically upon it; with the vigour thus gained it destroys the neighbouring cells and passes from one to another without further difficulty. The fungus hypha has the power of perforating the cuticle, but only in young and tender structures; old and hardened membranes could only be entered when the cuticle had been injured, or when it had gained strength by special saprophytic nutrition.

Whether in the case of *P. destructans* the toxin or cytase is the first excretory product I cannot say; the latter produces the first visible effect, and doubtless it prepares the way for a more rapid action of the toxin. But this is immaterial; though differing, as we have seen, in detail, the behaviour of *P. destructans* and *B. cinerea* is the same in principle. The main point is established that this bacterium has the power of destroying the living cells of the turnip, and, subsisting upon their dead contents, continues to work its way through the host, and it thus acts in precisely the same manner as one acknowledged parasitic fungus.

How far this kind of parasitism may be typical of bacterial diseases generally remains to be proved. Another form of *Pseudo-*

* A proof of the escape of the cell-sap was afforded by subjecting cells with a coloured sap to the action of this bacterium, in a hanging drop, when the coloured sap could be detected slowly percolating outwards through the cell-wall.

† M. Nordhausen, "Beiträge zur Biologie parasitärer Pilze," 'Jahrbücher für wissenschaftliche Botanik,' vol. 33, 1899.

monas producing a brown rot (not to be confused with *P. campestris*, E. F. Smith), which I am at present studying, works in a totally different manner; its action is very much slower, and the rapid swelling of the cell-wall, as described above, is not a conspicuous feature.

"The Influence of Varying Amounts of Carbon Dioxide in the Air on the Photosynthetic Process of Leaves and on the Mode of Growth of Plants." By HORACE T. BROWN, LL.D., F.R.S., and F. ESCOMBE, B.Sc., F.L.S. Received April 28,—Read May 29, 1902.

[PLATES 5—10.]

In a paper recently laid before this Society dealing with the physical processes which regulate the entry of atmospheric carbon dioxide into the leaves of plants,* we incidentally described a series of experiments relating to the rate of absorption of dilute gaseous carbon dioxide by surfaces of solutions of caustic alkali, when air containing definite small amounts of this gas is drawn over the liquid. Contrary to what might be expected from the perfect absorbing nature of the solution, and the known laws of gaseous diffusion, the amount of CO_2 absorbed by unit area of the liquid surface in unit time ceases sensibly to increase when a comparatively low velocity of the moving air current has been reached. This, however, only holds good when the proportion of CO_2 in the air stream is maintained quite constant, any slight variation in the amount at once affecting the rate of absorption. On investigation it was found that for dilutions of carbon dioxide lying between 0·6 part and 6 parts per 10,000 of air, the rate of absorption of the carbon dioxide *is strictly proportional to its partial pressure.*†

In determining the rates of gaseous diffusion of atmospheric carbon dioxide through multiperforate diaphragms extended over chambers containing perfect absorbents, the same relations between partial pressure of the gas and its absorption were found to hold good: under these conditions the amount of carbon dioxide passing through the

* 'Phil. Trans.,' B, 1900, vol. 193, p. 278.

† So accurately is this the case that the process, which is described in detail in the above-mentioned communication, may be used for determining the varying amounts of CO_2 in air without the necessity of measuring the volume of air which passes through the apparatus. It is merely necessary to pass the air over the absorbing surface at a sufficient rate to ensure maximum absorption, and to compare this amount of absorption in a given time with that produced from air of a known content of carbon dioxide, a process of standardisation which is done once for all with the apparatus. The ratios of the absorptions give at once the ratios of the partial pressures of the CO_2 in the two cases.

diaphragm in a given time is also directly proportional to the density of that gas in the moving stream of air which flows over the outer surface of the diaphragm.

But this latter case exactly defines the physical conditions under which atmospheric carbon dioxide enters the tissue of a living leaf, the multiperforate diaphragm being represented by the cuticle and epidermis, pierced with numerous stomata, and the inner absorbing chamber by the intercellular spaces of the parenchyma, bounded by the chlorophyll-containing cells in which the process of photosynthesis goes on (*loc. cit.*).

From these considerations one would be led to expect that under conditions favourable for photosynthesis, such as are fulfilled by the incidence of a sufficient amount of the right kind of radiant energy, a living leaf would be able to absorb amounts of carbon dioxide from the surrounding air which, within certain limits of concentration, are directly proportional to the partial pressures of that gas.

One essential condition would of course be that the actively assimilating organs of the leaf in which the photosynthesis takes place should act as *perfect* absorbers, that is to say, they must be able to deal with the carbon dioxide as fast as it is brought within their sphere of influence by the physical processes of diffusion. When this condition is no longer fulfilled, either through lack of a sufficient amount of radiant energy of the right wave-length reaching the active centres of photosynthesis, or through the natural limit of the metabolic activity of those living units having been reached in other directions, we should then expect the intake of the gas into the leaf to be no longer proportional to its partial pressure in the outer air.

We have been able to verify these deductions experimentally, and to show that a living leaf is really able, within certain limits, to respond to increased amounts of carbon dioxide in the air surrounding it, in such a manner as to indicate an approximate proportionality between the photosynthetic work it can accomplish, and the partial pressure the gas exercises in the air bathing the leaf surface.

In these experiments the leaves, which were in some instances still attached to the plant, had their laminæ enclosed in air-tight glazed cases, through which was aspirated a sufficiently rapid stream of air the volume of which was determined by means of carefully standardised meters. The carbon-dioxide content of the air was determined both before and after its passage over the leaf, and the area of the leaf was also accurately known. A full description of the apparatus employed will be given in a subsequent paper; it was so arranged that large volumes of air could be employed, containing known amounts of carbon dioxide either larger or smaller than the normal amount in the atmosphere.

Experiment I.—In this case comparative experiments were made on

two successive days in August, 1898, with two similar leaves, A and B of *Helianthus annuus*, whilst still attached to the plant. These were exposed to the strong diffused light of a clear northern sky under as nearly as possible identical conditions, with the exception of the composition of the air drawn through the cases.

Over leaf A was drawn normal air containing 2·8 parts per 10,000 of CO_2, whilst the air passing over leaf B contained 25·53 parts CO_2 per 10,000.

Leaf A.

Area of leaf	743·1 sq. cm.
Volume of air passed per hour, reduced to normal temperature and pressure	159·03 litres.
CO_2 content of air entering case	2·80 parts per 10,000
,, ,, leaving case	1·64 ,, ,,
Mean CO_2 content of air in contact with leaf during experiment	2·22 ,, ,,
CO_2 absorbed by leaf per hour	18·44 c.c.
,, per sq. metre per hour	248·2 c.c.

Leaf B.

Area of leaf	863·75 sq. cm.
Volume of air passed per hour reduced to normal temperature and pressure	72·7 litres.
CO_2 content of air entering case	25·30 parts per 10,000
,, ,, leaving ,,	4·12 ,, ,,
Mean CO_2 content of air in contact with leaf during experiment	14·82 ,, ,,
CO_2 absorbed by leaf per hour	155·7 c.c.
,, per sq. metre per hour	1802·8 c.c.

It is manifest that if we wish to determine the relation of the partial pressures of carbon dioxide to the rate of intake of that gas into the leaf, we must employ the values representing the mean carbon-dioxide content of the air in contact with the leaf during the experiment, which may be taken as the arithmetical mean of the composition of the entering and emergent air. In the above experiment we obtain the following relations :—

Ratio of partial pressures of CO_2 in A and B, 2·22 : 14·82 or **1 : 6·6**.

Ratio of CO_2 absorbed per sq. metre of Leaf A and B in 1 hour, 248·2 : 1802·8 = **1 : 7·2**.

Thus by increasing the amount of CO_2 in the air passing over the leaf about sevenfold, we have, under similar conditions of illumination, increased the photosynthetic power of the leaf by a little more than the same amount.

Experiment II.—In this instance two similar leaves, A and B, of *Helianthus annuus* (cut from the plant, and with their petioles immersed in water) were exposed on August 25, 1899, to sunlight under a

thin white canvas screen, the separate glazed cases containing the leaves being placed side by side under exactly similar conditions. The duration of the experiment was 4 hours.

Through case A ordinary air was passed, and through case B air enriched with CO_2 up to 13·1 parts per 10,000 :—

Leaf A.

Area of leaf	336·4 sq. cm.
Air passed in 4 hours (N.T.P.)	378·82 litres.
„ 1 hour	94·70 „
CO_2 content of air entering case	2·80 parts per 10,000
„ „ leaving „	1·70 „ „
Mean CO_2 content of air in contact with leaf during experiment	2·25 „ „
CO_2 absorbed by leaf per hour	10·39 c.c.
„ „ per sq. metre per hour	309 c.c.

Leaf B.

Area of leaf	312·7 sq. cm.
Air passed in 4 hours (N.T.P.)	325·59 litres.
„ 1 hour „	81·39 „
CO_2 content of air entering case	13·1 parts per 10,000
„ „ leaving „	6·8 „ „
Mean CO_2 content of air in contact with leaf during experiment	9·95 „ „
CO_2 absorbed by leaf per hour	51·25 c.c.
„ per sq. metre per hour	1639 c.c.

The ratio of partial pressures of CO_2 in A and B is 2·25 : 9·95 = **1 : 4·4**. The ratio of CO_2 absorbed per sq. metre per hour in A and B is 309 : 1639 = **1 : 5·3**, so that there is again a fair correspondence of the ratios.

Many other similar experiments were made, of which the two quoted are typical examples. In all cases where the illumination of the leaf was good, although the amount of intake of CO_2 into the leaf was approximately proportional to the increased partial pressure, the photosynthetic work of the leaf was always somewhat in excess of what might be expected from the increased amount of CO_2.

This is probably due to the fact that under the particular conditions of the experiment the air which was more highly charged with the gas also contained a little more moisture than the ordinary air. Under these circumstances the stomata of the B series would tend to open a little more than those of the A series, a fact which would in itself account for the correspondence between partial pressures and intake not being so perfect as it would otherwise have been, since, other things being equal, the intake is proportional to the linear dimensions of the openings.

In the two following experiments, where the illumination of the leaves was of *low intensity*, the differences are in the other direction,

the increased amounts of CO_2 in the air passed over the leaf being evidently in excess of the power of the assimilatory centres to deal with them. In such cases the ratios of intake are necessarily considerably lower than the ratios of the partial pressures.

Experiment III. Under Insufficient Illumination.—In this instance the day (August 29, 1899) was very cloudy, with rain falling during nearly the whole time.

Cut leaves of *Helianthus annuus* A and B were placed in adjoining cases as in the other experiments.

Leaf A.

Area of leaf	275·9 sq. cm.
Air passed in 3¼ hours (N.T.P.)	287·94 litres
,, 1 hour	88·5 ,,
CO_2 content of air entering case	2·80 parts per 10,000
,, ,, leaving ,,	1·70 ,, ,,
Mean CO_2 content of air in contact with leaf during experiment	2·25 ,, ,,
CO_2 absorbed by leaf per hour	9·7 c.c.
,, per sq. metre per hour	351 c.c.

Leaf B.

Area of leaf	411·2 sq. cm.
Air passed in 3¼ hours	248·17 litres.
,, 1 hour	76·3 ,,
CO_2 content of air entering case	21·3 per 10,000
,, ,, leaving ,,	11·5 ,,
Mean CO_2 content of air in contact with leaf during experiment	16·4 ,,
CO_2 absorbed by leaf per hour	74·8 c.c.
,, per sq. metre per hour	1819 c.c.

Ratio of partial pressures of CO_2 in A and B, 2·25 : 16·4 = **1 : 7·2**. Ratio of CO_2 absorbed per square metre per hour in A and B, 351 : 1819 = **1 : 5·1**.

Experiment IV.—A similar experiment under *insufficient illumination* on leaves of *Catalpa bignonioides*, August 31, 1899. The sky was very dark, with some rain, and only a few gleams of partial sunlight near the close of the experiment.

Leaf A.

Area of leaf	417·6 sq. cm.
Air passed in 4 hours (N.T.P.)	304·33 litres.
,, 1 hour	76·08 ,,
CO_2 content of air entering case	2·80 per 10,000
,, ,, leaving ,,	1·30 ,,
Mean CO_2 content of air in contact with leaf during experiment	2·05 ,,
CO_2 absorbed by leaf per hour	11·41 c.c.
,, per sq. metre per hour	273·2 c.c.

Leaf B.

Area of leaf	433·6 sq. cm.
Air passed in 4 hours (N.T.P.)	250·1 litres.
,, 1 hour	62·51 ,,
CO_2 content of air entering case	16·1 per 10,000
,, ,, leaving ,,	9·2 ,,
Mean CO_2 content of air in contact with leaf during experiment	12·65 ,,
CO_2 absorbed by leaf per hour	43·12 c.c.
,, per sq. metre per hour	994·4 c.c.

Ratio of partial pressures of CO_2 in A and B, 2·05 : 12·65 = **1 : 6·1**. Ratio of CO_2 absorbed per square metre per hour in A and B, 273·2 : 994·4 = **1 : 3·6**.

These experiments, and many others of a similar nature which might be quoted, indicate beyond a doubt that, at any rate for a short period, the photosynthetic functions of the leaf lamina are capable of being intensified by increasing the amount of carbon dioxide in the surrounding air, and that under favourable conditions the response of the leaf in this direction is approximately directly proportional to the amount of CO_2 present.

Experiments of this nature are necessarily limited to comparatively short periods, and give us no information as to how far the plant, as a whole, will respond to such changes in its atmospheric environment. When first drawing attention to these facts in 1899* it was pointed out by one of us that we were not justified, without direct experiment, in concluding that the plant would be able to avail itself indefinitely of the increased amount of plastic carbohydrate material formed in its leaves under these artificial conditions, and that translocation, metabolism, and growth may have become so intimately correlated that the perfect working of the entire plant may only be possible in an atmosphere containing the normal amount of three parts of CO_2 per 10,000.

In approaching this question experimentally we were led to make certain preliminary experiments with a view to seeing how far slightly increased amounts of carbon dioxide in the air would affect the actual dry weight of plants grown in such an atmosphere. Should there be any influence on the plant throughout its growth in any way comparable in magnitude with that of the increased photosynthesis in leaves it could not possibly escape detection, even when using atmospheres containing only two or three times the normal amount of CO_2.

Experiment V.—Two seedling plants of *Vicia Faba* were chosen which had been grown in pots, and which were of the same age and as nearly similar in appearance as possible. Above each of the pots was placed a circular metal tray with a large central aperture, and an annular rim which formed a water-seal for a large inverted glass beaker. The stem

* Presidential Address, British Association, Section B, Dover.

of each of the young plants was passed through a split cork fitted into the central hole of the tray, and was luted air-tight with a soft mixture of vaseline and paraffin. Two narrow tubes also passed through the tray, one of which was cut short inside whilst the other extended to the top of the glass covering the vessel. A slow current of moist air was aspirated through each inverted beaker. In one case ordinary air was used, containing from 2·8 to 3 parts of CO_2 per 10,000, whilst in the other the air stream was enriched with CO_2 to the extent of 5·4 parts per 10,000 by passing through a small glass tower containing marble, over which a regulated amount of very dilute hydrochloric acid was slowly dropped. In this latter instance the air-stream was passed through a wash-bottle containing a solution of sodium bicarbonate before it entered the chamber containing the plant. In the control experiment with ordinary air only water was used in the wash-bottle.

The extra carbon dioxide was only supplied during the daytime, whilst during the night both chambers were supplied with ordinary air. The experimental chambers were placed side by side in a well-lighted greenhouse under exactly equal conditions of temperature, direct sunlight being prevented from reaching the plants.

The experiment in this instance lasted for 11 days, extending from June 29 to July 10, 1899.

The plants, which were almost identical in appearance, were then carefully washed out of their pots, and with their roots were dried and weighed.

Dry Weight of Plants.

A. Grown in normal air	0·856 gramme.
B. Grown in air containing double the normal amount of CO_2	0·843 ,,

Experiment VI.—Another similar experiment with *Vicia Faba.*

In this case beans of as nearly an equal weight as possible were germinated in pots, and a selection from the seedlings was made of well-developed plants of similar appearance.

The CO_2 was in one case increased to twelve parts per 10,000, *i.e.*, four times the normal amount, ordinary air being passed over the other plant. The experiment lasted 10 days, from August 19th to August 29th, 1899.

Dry Weight of Plants.

A. Grown in normal air	0·872 gramme.
B. Grown in air containing four times the normal amount of CO_2	0·814 ,,

The *foliar area* was also measured in this instance, with the following result :—

A.	Area of leaves on plant in normal air	66·9 sq. cm.
B.	,, ,, in air with four times normal amount of CO_2...	55·6 ,,

Similar results were also obtained with young plants of *Helianthus annuus.*

The experiments indicate that the plants were certainly not stimulated to increased growth by somewhat increasing the amount of CO_2 in the surrounding air. The evidence in fact points in the other direction, *i.e.*, towards a slight diminution in the increment of dry weight, and to a less development of foliar area. There were also indications of certain morphological differences, which assumed some importance in the light of subsequent experiments. The plants grown in air slightly enriched with CO_2 had not only smaller leaves than the controls, but these leaves were of a distinctly darker green, and the internodes of the plants were decidedly shorter.

The results obtained with these preliminary experiments now induced us to extend our observations to a larger number of species, and arrangements were consequently made to carry out a series of experiments on a large scale, and under conditions which would admit of the plants being kept under observation for a much longer period of time.

For this purpose a small greenhouse adjoining the Jodrell Laboratory was kindly placed at our disposal by the Director. This was divided into two compartments, each of about 380 cubic feet capacity, by means of a glazed partition, which was made quite air-tight. In one of these, compartment A, the experimental plants were grown in normal air, whilst the air of compartment B could be enriched to any desired extent with carbon dioxide supplied from a receiver of the liquefied gas placed in an adjoining building. The gas before entering the greenhouse was bubbled through a wash-bottle at a rate which previous experiments had shown to be necessary in order to keep the composition of the air approximately constant. Frequent analyses of the air in both compartments were made as a check.

The current of carbon dioxide was started each day at 6 A.M., and continued until 6 P.M., both greenhouses being closed during the day, and the glass top and sides of the houses were white-washed over in order to exclude direct sunlight, whilst at the same time allowing plenty of illumination. During the night both greenhouses were opened and well ventilated from the outside, so that for 12 hours out of the 24 the atmospheric conditions were identical. Care was taken to maintain the temperature, degree of illumination, and the hygroscopic state of the two compartments as nearly as possible identical, the only point of difference being in the composition of the air.*

* The plants were grown in a rich garden soil containing all the mineral and nitrogenous constituents necessary for luxuriant growth.

Two series of experiments were made, one in the summer of 1900, and the other in the summer of 1901.

The composition of the air as regards carbon dioxide differed very much in these two experiments, in one case the air being enriched with about 5·5 to 6 *per cent.* of the gas, *i.e.*, 180 *to* 200 *times* the normal amount, whilst in the other case the mean CO_2 content was only 0·114 *per cent.*, i.e., *between three and four times* the normal amount.

Contrary to what might have been expected, the general results, compared with those obtained with the control plants in ordinary air, were practically identical in the two sets of experiments, notwithstanding the large range of something like 1 to 50 in the CO_2 content of the enriched air.

Since the greater interest attaches to the experiments with the smaller increment of CO_2, we shall confine our attention almost entirely to this set in the remarks which follow.

The plants taken for this experiment were as follows :—

(1.) 8 plants of *Cucurbita Pepo* (Italian Gourd).
(2.) 8 ,, ,, *Impatiens platypetala.*
(3.) 6 ,, ,, *Nicotiana affinis.*
(4.) 6 ,, ,, *Nicotiana sylvestris.*
(5.) 10 ,, ,, *Begonia gracilis.*
(6.) 6 ,, ,, *Solanum atropurpureum.*
(7.) 12 ,, ,, *Kalanchoë Welwitschii.*
(8.) 6 ,, ,, *Fuchsia*, var. with light coloured leaves.
(9.) 6 ,, ,, *Fuchsia*, var. with darker leaves.

One-half of each set was placed in compartment A, the other half in compartment B, the plants being carefully matched as regards size and general appearance, and arranged symmetrically as regards the glass division separating the two compartments,

Set A in ordinary air (controls).

Set B in air enriched with CO_2.

The experiment was commenced on May 13, and continued until July 29, *i.e.*, for a period of 77 days.

The mean content of the air in CO_2 *during the daytime* was :—

In compartment A, 3·29 parts per 10,000.
,, ,, B, 11·47 ,, ,, ,,

So that the atmosphere of compartment B contained during the daytime about three and a-half times more CO_2 than that of compartment A.

A careful record was made of the differences in appearance of the two sets of plants on June 10, June 29, and July 13, that is to say, at 28, 47, and 61 days from the commencement of the experiment. The results are given in detail in the Appendix to this paper, and may be summarised as follows :—

The effect of an increased amount of CO_2 in the air becomes in most cases apparent within a week or 10 days from the commencement of the experiment, and rapidly increases as times goes on. There is a marked difference induced in the habit and general appearance of most of the plants owing to a stimulation of all axial growth, accompanied by a more or less pronounced shortening and thickening of the internodes. Usually, but not in all cases, there is an increased number of the internodes, so that the height of the two contrasted sets remains much about the same, but the chief difference of general habit is brought about by the development throughout the plant of secondary axes in the axils of the leaves, thus giving the plants grown under the influence of increased CO_2 a denser and more bushy appearance. This was particularly noticeable in the *Fuchsias*, especially the dark-leaved variety, in which every axil bore a shoot, and frequently extra axillary ones. Adventitious shoots were also developed rather freely at the base of the plants.

The leaf area of the plants under the influence of increased CO_2 was generally found to be much reduced, not so much by the formation of a less number of leaves as by the reduction in area of the individual leaves. This was found to be extreme in the case of the dark-leaved *Fuchsias*, and it was also very marked in the second crop of the leaves of *Impatiens*. There was also produced in many of the plants a marked inward curling of the leaves, the extremes in this direction being found in the *Begonias* and *Fuchsias*. In the dark-leaved variety of *Fuchsia* the leaves were curled inwards like a watch-spring, which would doubtless tend to reduce excessive photosynthesis by preventing the normal amount of light from reaching the chloroplasts. This change of habit may in fact be regarded as an attempt on the part of the plant to adapt itself to its abnormal atmospheric surroundings.

The extra CO_2 in several cases induced a deeper green colour in the leaf, and in all other parts of the plant where chlorophyll was present. This was particularly noticeable in the second crop of leaves developed on the *Impatiens*, in the *Begonias*, and in the darker-leaved *Fuchsias*.

On July 19, the Sachs test for starch was applied to the leaves of the two varieties of *Fuchsia*, *Cucurbita Pepo*, and *Impatiens platypetala*. In all cases the leaves taken from the plants grown with increased CO_2 in the air showed a much larger accumulation of starch than did the leaves of the control plants. These differences were the most strongly marked in the leaves of *Impatiens*, which became quite black with the test.

It was, however, in the development of the *reproductive organs* of the two sets of plants that the most striking and important differences were found. Whilst the control plants in ordinary air flowered and in some cases fruited luxuriantly, in the corresponding plants sub-

mitted to air containing 11·4 parts per 10,000 of CO_2, *inflorescence was almost totally inhibited.* With the exception of one or two sickly looking flowers on the *Begonias*, not a single flower-bud opened on any of the plants of this set. The plants of *Impatiens*, *Kalanchoë*, and of the darker-leaved *Fuchsias*, did not even produce a flower-bud, whilst in the *Nicotiana*, *Cucurbitas*, and lighter-leaved *Fuchsias*, the small flower-buds which commenced to form were completely shed long before the time of opening.

The plants which appeared to be most sensitive to the action of the extra CO_2 in the earlier stages of growth were those of *Impatiens platypetala*, which from the commencement presented a strong contrast to the healthy appearance of the controls. They lost nearly all their leaves, and the bare stems shed many of their internodes, which were cut off successively just above the nodes. By June 29, however, the plants had begun to recover, and, by putting out a second growth of small and very dark green leaves, indicated a certain limited adaptation to their abnormal atmospheric environment.

So far only the more striking visible differences between the two sets of plants have been described. It is highly probable that these will be found correlated with equally well-marked differences of anatomical structure. This part of the enquiry has been kindly undertaken by Professor J. B. Farmer and Mr. S. E. Chandler, who will embody their results in a separate communication.

In another series of experiments, which we carried out on similar lines, the air of compartment B was enriched with carbon dioxide to the extent of 6 *per cent.*, that is to say, up to about 200 times the normal amount. The plants used in this experiment were as follows:—

Nicotiana sylvestris.
„ *tomentosa.*
Solanum giganteum.
Fuchsia.
Ricinus communis, green variety.
Ricinus communis, red variety.
Chrysanthemum Broussonettii.
Begonia multiflora.
Kalanchoë rosea.
Cucurbita Pepo.

The experiment extended from June 3 to August 26, and the general results both in the direction and amount of change of habit induced in the plants were so very similar to those induced in the plants with only three and a-half times the normal amount of CO_2, as to require no further special description. The results are, however, valuable as indicating that the observed differences cannot be due to any *direct* poisonous influence of the carbon dioxide, otherwise we should certainly expect a marked difference to be produced by increasing the amount of CO_2 from 11·4 parts per 10,000 to 600 parts per 10,000, *i.e.*, more than fiftyfold, which was not the case to any appreciable extent.

The direction in which we must search for the true explanation of

the effect is probably indicated by the experiments on leaves described in the early part of the paper, where it was shown that the amount of photosynthesis in the leaf lamina is, within certain ill-defined limits, a function of the partial pressure of the CO_2 in the surrounding air.

In the first series of experiments in the greenhouse, where this partial pressure was maintained at about three and a-half times the normal, the plants for a certain limited period must have been manufacturing carbohydrate material within their chloroplasts at least three and a-half times faster than those in normal air, and, although this rate of photosynthesis would perhaps not be maintained for very long, yet there would always be a general tendency for the carbohydrate supply in the leaves to be kept up to a higher point than in the controls grown in ordinary air, a fact which was shown by the leaves of set B always being gorged with starch.

Since it is quite certain that this increased photosynthesis does not to any material extent contribute to the increase of dry weight of the plants, we can only conclude that the transformation, translocation, and general metabolism of the leaf-reserves under these conditions cannot keep pace with the increased tendency to produce an extra amount of plastic material from the atmosphere. Moreover, it is clear that the whole mechanism of the plant on which normal nutrition depends has its parts so completely and accurately correlated that any slight increase in the composition of the surrounding air which favours increased photosynthesis destroys the adjustment of the various parts and results in a more or less abnormal development of the plant. That any such disturbance of the economy of the plant should profoundly modify the reproductive functions, might perhaps have been expected.

It is somewhat remarkable to find that all the species of flowering plants, without exception, which have been the subject of experiment, appear to be accurately "tuned" to an atmospheric environment of 3 parts of CO_2 per 10,000, and that the response which they make to slight increases in this amount, are in a direction altogether unfavourable to their growth and reproduction. It is not too much to say that a comparatively sudden increase of carbon dioxide in the air to an extent of but two or three times the present amount, would result in the speedy destruction of nearly all our flowering plants.

To a certain extent we may regard the facts recorded in this paper as indicating that the composition of our atmosphere as regards its carbon dioxide, has remained constant, or practically constant, for a long period of time, but they leave altogether untouched the question of any variations of a secular kind. All we are justified in concluding is that if such atmospheric variations have occurred since the advent of flowering plants, they must have taken place so slowly as never to outrun the possible adaptation of the plants to their changing conditions.

Appendix.

Details of the experiments made on Plants in Greenhouse. One set A grown in ordinary air, the other set B grown in air containing 11·47 *parts of* CO_2 *per* 10,000.

For list of these plants, see p. 407.

The experiment was commenced on May 13.

(1.) *Cucurbita Pepo.*

June 10.—The only difference observable between the two sets of plants, was that a much less number of flower-buds was developing on the plants of set B in air with extra CO_2.

June 29.—The development of the flower-buds on the plants grown in extra CO_2 was now completely arrested.

July 13.—A well marked difference between the two sets. Those of set A, in ordinary air, had formed flower-buds in all the axils and were flowering luxuriantly. The plants of set B (extra CO_2) were much the taller, owing to an increased number of internodes. There was also a considerable tendency to the production of secondary axillary shoots instead of flower-buds, whilst all the flower-buds which had formed had long since fallen off before they were fully developed.

(2.) *Impatiens platypetala.*

June 10.—All the four plants of set B (extra CO_2) were very unhealthy in appearance, having lost nearly all their leaves, and with no vestige of a flower-bud. The controls of set A in ordinary air were very healthy in appearance, and were bearing numerous flowers. (See Plate 5.)

June 29.—The plants of set B (extra CO_2), after losing their leaves, proceeded to shed certain internodal parts of their axes just above a node, and this process was in some cases repeated for some distance down the axis. On this date the plants were showing signs of recovery, and had produced a second crop of small and very dark green leaves.

The control plants A, in ordinary air, were normal in every way, and had just finished flowering.

July 13.—There was still a very marked difference between the two sets. Those of set A (ordinary air) were somewhat the taller, the axes being more divergent and the leaves not so closely set. The leaves were also larger and lighter green than those of B. Set B (extra CO_2). The plants had now recovered, and acquired a fairly healthy appearance. There were no flower-buds formed, and the leaves (second crop) were much smaller and darker in colour than those of set A.

(3.) *Nicotiana affinis.*

June 10.—Each set of plants had thrown up a flowering axis which in set B (extra CO_2) was not quite so fully developed, but whereas the flower-buds of the control plants in ordinary air had, in several cases, opened normally, the buds of set B (extra CO_2) were much fewer in number, very small, and showed no signs of opening. (See Plate 6.)

June 29.—The difference was now very striking. The plants in ordinary air had flowered well, but the set B, subjected to extra CO_2, although possessing well-developed flowering axes, had borne no flowers at all, the buds having been arrested in development.

July 13.—There were the same well-marked differences between the sets as when last noted. The plants in extra CO_2 had now thrown up several secondary axes from their base.

(4.) *Nicotiana sylvestris.*

June 10.—Set A, grown in ordinary air, were further developed than those of B, and possessed larger leaves.

June 29.—The same difference as on June 10. No flowers formed on plants in extra CO_2, whereas the controls flowered freely.

July 13.—The same remarks apply as on June 29.

(5.) *Begonia gracilis.*

June 10.—The six plants of set A, grown in ordinary air, were healthy and had flowered well. Those of set B (extra CO_2) appeared very unhealthy, and in only one instance showed any flowers, which were small, greenish, and altogether abnormal. (See Plate 7.)

June 29.—Whilst the control plants grown in ordinary air were in full flower, those of set B grown in extra CO_2 had only a few small, unopened buds, which dropped off before they opened.

The general appearance of the plants grown in extra CO_2 had now become fairly healthy, but there was an abnormal tendency for their leaves to curl downwards and inwards.

July 13.—A very marked difference in the two sets. Whilst the height was about the same the axial organs of set B, grown in extra CO_2, were much stouter than those of the controls, whilst the internodes were shorter and more numerous. The plants grown in ordinary air had a more spreading and less dense habit, whilst the leaves of set B (extra CO_2) had still a marked tendency to curve downwards and inwards, their colour being on the whole a darker green.

Whilst the plants in ordinary air had flowered and fruited well, those grown in extra CO_2 had either cast their buds entirely, or in some few cases these had developed into small, sickly, abnormal flowers, which in no case fruited.

(6.) *Solanum atropurpureum.*

June 10.—The leaves of set B (extra CO_2) were somewhat curved downwards, and the axes were distinctly higher than those of the controls. The spines on the leaves and stems were not so fully developed as on the control plants in ordinary air. (See Plate 8.)

June 29.—It was very remarkable to note how the axis of each plant of set B (extra CO_2) had become elongated as compared with the controls. There was also a less marked development of spines in the former case.

July 13.—There was now a very marked difference between the habits of the two sets. Without exception the plants of set B (grown in extra CO_2) were much taller. The height of the three plants in extra CO_2 was found to be from 23 to 24 inches, and the number of internodes 23 to 24, the average length of the internodes, therefore, being about 1 inch. In set A, grown in normal air, the height was about 14 inches, and the number of internodes about 16; so that the average length of the internodes in this latter case was about 0·8 inch. There were more internodes on the plants grown in extra CO_2, and they were somewhat longer.

There were about twice as many leaves on the plants grown in ordinary air, and the spinous hairs were more closely crowded together on these control plants, were stronger in growth, and of a deeper purple colour. No flowers had been developed on the plants of set B, grown in extra CO_2.

(7.) *Kalanchoë Welwitschii.*

June 10.—The control plants of set A in ordinary air were the better grown, and possessed larger leaves than those of set B (extra CO_2).

June 29.—There was but little difference in the height of the two sets, but the contrast as regards the size of the leaves was still very marked.

July 13.—The differences in the direction already indicated were still more pronounced.

(8.) *Fuchsia.*—(a) *Variety with Lighter Leaves.*

June 10.—The plants of series B, grown with extra CO_2, were not quite so tall as the controls, and their leaves were smaller. The flower-buds of this set commenced to develop, but soon dropped off after becoming very green.

June 29.—The habit of the plants grown in extra CO_2 was a little denser than that of the controls. Whilst the controls (in ordinary air) were flowering copiously, the flower-buds of set B (in extra CO_2) became green and were shed before they opened.

July 13.—In no single case had a bud opened on the plants grown in air containing increased amounts of CO_2, and their denser habit was still more strongly marked. The leaves were also smaller, of darker green and with a tendency to curl up, whilst the internodes were a little stouter, and shorter, and the colour of the axes was a little more pink.

(9.) *Fuchsia.*—(b) *Darker-leaved Variety.*

June 10.—There was already a marked difference between the two sets as regards general habit and appearance. The control plants in normal air possessed long and slender axes and were freely developing flower-buds. Set B, grown with extra CO_2, had a very abnormal appearance; their main axes were much shorter, whilst the leaves were extremely small and curled inwards; there was also a great development of axillary shoots, and adventitious ones at the base of the stems. (See Plate 9.)

June 29.—The above difference had become still more accentuated. No flowers had been formed on set B, grown with extra CO_2 in the air.

July 13.—The differences between the two sets were now extreme. The plants of set B (extra CO_2) had a most abnormal appearance. Their leaves were all extremely small, of a much darker green, and were rolled up in an inward spiral like that of a watch-spring. There was also a profuse development of secondary axes in the axils of most of the leaves, from the base of the primary axis up to its apex. This habit of growth gave the whole plant a much denser appearance than that of the control plants grown in ordinary air. The internodes were on the whole shorter and stouter than those of the controls, and differed markedly in colour: whereas those of the controls had the ordinary blood-red appearance, the internodes of the plants grown in extra CO_2 were pinkish-green. No flowers had developed on the plants of set B, grown in extra CO_2. (See Plate 10.)

DESCRIPTION OF THE PLATES.

Plate 5.—*Impatiens platypetala.*

Under experiment for 28 days.

A. Plants grown in ordinary air.

B. ,, in air containing 11·4 parts per 10,000 of CO_2.

Plate 6.—*Nicotiana affinis.*

Under experiment for 28 days.

A. Plants grown in ordinary air.

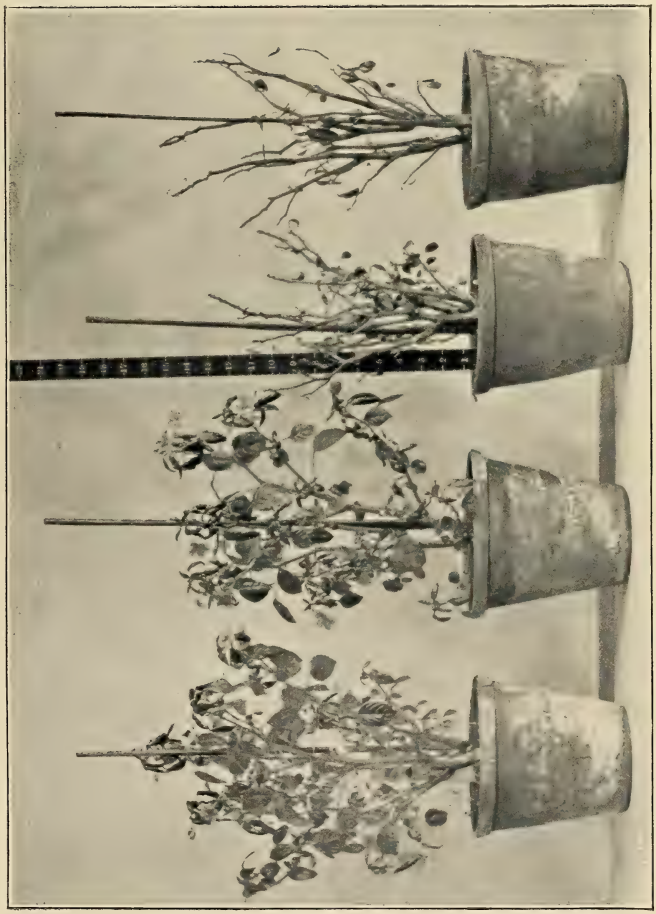
B. ,, in air containing 11·4 parts of CO_2 per 10,000.

Plate 7.—*Begonia gracilis.*

Under experiment for 28 days.

A. Plants grown in ordinary air.

B. ,, in air containing 11·4 parts of CO_2 per 10,000.



A B

PLATE 5.

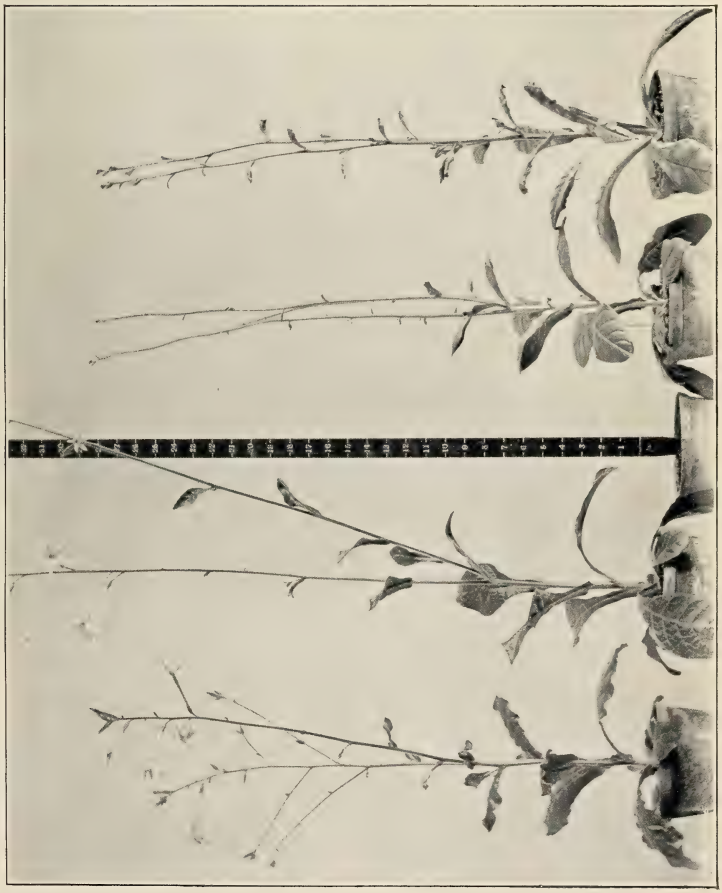
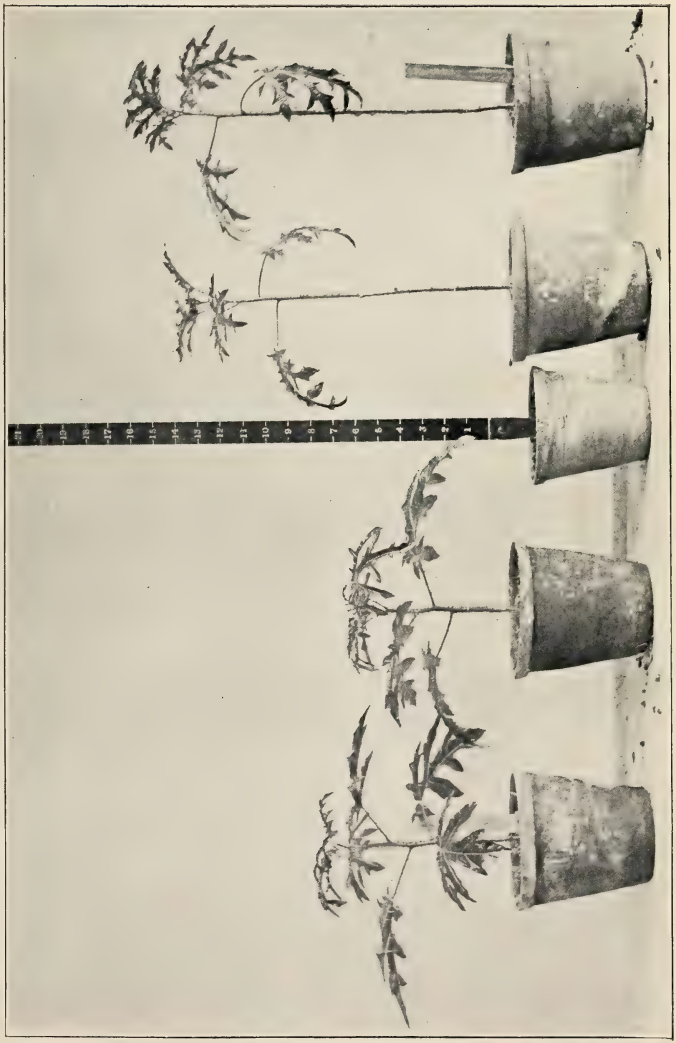


PLATE 6.

A B

PLATE 7.



A B

PLATE 8.

A B

PLATE 9.

A B

PLATE 10.

Plate 8.—*Solanum atropurpureum.*
Under experiment for 28 days.
A. Plants grown in ordinary air.
B. „ in air containing 11·4 parts of CO_2 per 10,000.
Plate 9.—*Fuchsia.* Dark-leaved variety.
Under experiment for 28 days.
A. Plants grown in ordinary air.
B. „ in air containing 11 ·4 parts of CO_2 per 10,000.
Plate 10.—*Fuchsia.* Dark-leaved variety.
Under experiment for 54 days.
A. A plant grown in ordinary air.
B. „ in air containing 11 ·4 parts of CO_2 per 10,000.

N.B.—The scale on the rod is in inches.

"On the Influence of an Excess of Carbon Dioxide in the Air on the Form and Internal Structure of Plants." By J. BRETLAND FARMER, D.Sc., F.R.S., Professor of Botany in the Royal College of Science, London, and S. E. CHANDLER, A.R.C.S. Received May 6,—Read May 29, 1902.

The plants which form the subject of the present enquiry were kindly handed over to us by Messrs. Horace Brown and F. Escombe. They were preserved in alcohol, and had previously served as the material on which the researches of these investigators on the influence of varying amounts of carbon dioxide in the air on the photosynthetic processes of leaves and on the mode of growth of plants* had been conducted.

The series, consisting of the following five plants, viz., *Kalanchoë Welwitschii*, *Solanum atropurpureum*, *Begonia gracilis*, *Impatiens platypetala*, *Fuchsia*, sp., included in every case specimens which had been grown in a greenhouse in ordinary air containing 3·29 parts of carbon dioxide in 10,000 volumes of air, and others that had been cultivated under conditions as similar as possible except that the atmosphere contained about 3·5 times (11·47 per 10,000) the amount of carbon dioxide normally present in ordinary air. For the sake of brevity we shall refer to them as the *air* or control and as the CO_2 plants respectively.

We directed our attention more especially to the following points:—

(1.) The relative dimensions of the internodes.

* *Cf.* H. Brown and F. Escombe, "On the Influence of Varying Amounts of Carbon Dioxide in the Air on the Photosynthetic Processes of Leaves and on the Mode of Growth of Plants," 'Roy. Soc. Proc.,' vol. 70, in which full details are given as to the methods of experiment and the external appearances presented by the plants.

(2.) The average relative areas of the leaves.

In order to obtain this estimate the leaves were traced on paper, the pattern then cut out, and the average weights of each series obtained.

(3.) The number of stomata per unit of area of leaf surface, and also the *relative number of stomata and epidermal cells* per unit area. The object of the latter determination is to endeavour to ascertain whether any observed alteration in the total number of stomata per unit of area should be ascribed to an alteration in the degree of development reached by the epidermal cells, or whether it is to be referred to a direct increase or decrease of the stomata as the result of the influence of the added carbon dioxide.

(4.) The anatomical differences in the stems and leaves.

(5.) The relative amount of starch and other cell-contents in the two series.

The drawings which illustrate this communication are all drawn carefully to one scale, and are therefore strictly comparable for purposes of measurement.

Kalanchoë Welwitschii.—The average length of the four youngest internodes in the air plant as compared with those of the CO_2 plant is in the proportion of 1·0 : 0·75, taking those of the air plant as unity. If the development of the internodes in the two series be traced backwards, it is found that the CO_2 plant attains to its final internodal length sooner than does the air plant. Thus in one example, whilst the full length was reached in the CO_2 plant at the third or fourth internode, it was not till the fifth that further extension ceases in the air plant. The effect of this is to further emphasise the stunted appearance characteristic of the CO_2 series.

The leaves differ greatly in the two sets, being larger (1·0 : 0·17), thicker (1·0 : 0·7), and more broadly ovate and serrate in the air-grown specimens. As regards the stomata, the guard cells were of approximately equal size in both series, but were always widely opened in the CO_2 plants, whilst they were closed in the control. This points to a permanent alteration of form, seeing that the material throughout was already killed and preserved in spirit. A similar difference in the appearance of the guard cells we found to be very constantly exhibited by all the other species examined by us.

The numerical proportion of the stomata in the two series per unit of leaf (under) surface was about 1·0 : 1·5 (fig. 1, A and B); but this is largely due to the decreased size of the rest of the epidermal cells in the plant treated with carbon dioxide. This is proved by ascertaining the ratios between the number of epidermal cells and stomata in the two series respectively; it is found to be 100·0 : 7·3 for the air plant and 100·0 : 7·1 for the CO_2 specimen. It thus appears that in *Kalanchoë* there is no material disturbance of the normal

proportion existing between the epidermal cells and stomata as a consequence of the added carbon dioxide. It will, however, be clear that the small leaves of the latter series do possess, area for area, a far greater number of stomata than do the leaves of the control plants.

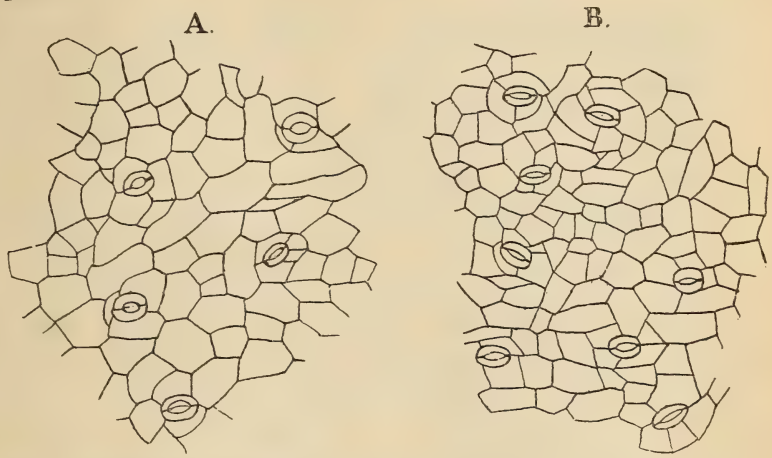


FIG. 1. *Kalanchoë Welwitschii*, epidermis of the underside of the leaf. A, air-; B, CO_2-plant.

In the anatomical or internal structure the differences are not so great as might perhaps have been anticipated. They chiefly affect the amount of xylem vessels and tracheids produced, and also the degree of differentiation of the mechanical tissues.

The vascular bundles are commonly less in number in the CO_2 than in the control plants, and the xylem of the former is always more parenchymatous in character than is that of the latter series in which the tracheids and vessels are tolerably uniformly distributed (see fig. 2). The phloem is, however, equally well developed in the bundles of both series. The stem of the air-grown plants is provided with a well-marked hypodermal collenchyma and pericyclic sclerenchyma, and in both respects it surpasses the CO_2 plant in the degreee of development marked by these tissues. We consider that the reduction in the conducting elements of the xylem is probably to be chiefly correlated with the diminished leaf surface, and the consequent lower transpiration, for the total number of stomata on the small leaves is far short of that present on the normal plants. The comparative poverty in mechanical tissue is perhaps partly connected with nutritional disturbances, and partly with the diminished weight of the growing organs of the plant. For it is known that the degree of differentiation of mechanical tissues is subject to modifications depending on the amount of stress or strain imposed upon the growing organs.

As regards the structure of the leaf, the character of the mesophyll does not differ, except in amount, in the two cases.

The roots showed no difference whatever in structure in the two series.

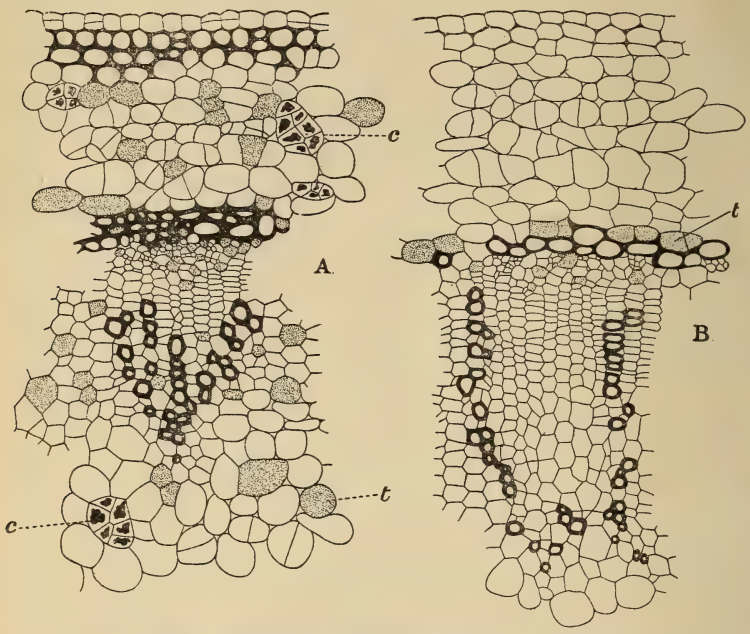


FIG. 2. *Kalanchoë Welwitschii*, transverse section of the stem. A, air- ; B, CO_2-plant.

Kalanchoë stands alone amongst the plants we examined in one respect. The specimens which had been exposed to the additional carbon dioxide exhibited no increase in the amount of starch present in their leaves or cauline parenchyma. But a difference was indicated between the metabolic proceeds of the CO_2 and air plants respectively in two other particulars. Firstly, the air plants contained large quantities of tannin-cells, especially in the vicinity of the vascular bundles. These were very much fewer and far less striking in the CO_2 plant. Again, the air plants possessed crystal-containing cells of remarkable form in the pith. These occur in groups, each group having arisen by the division of a single mother cell, the limits of which can still be traced. Each cell thus produced contains crystals of calcium oxalate. These special cell groups were entirely absent from the CO_2 plants; and when crystals occur in them, as they do very occasionally, they are found in ordinary unspecialised cells of the ground tissue.

Solanum atropurpureum.—The general tendency towards a stunted

appearance on the part of the CO_2 plant is also manifested by this species.

Comparing the total average length of the five youngest internodes in each specimen, the proportion was found to be 1·00 : 0·65, but nearer the base of the specimens this relation was reversed. The amount of material at our disposal did not, however, enable us to determine whether this is to be regarded as an accidental variation, though we incline to think it is not so. Possibly it may be connected with the fact, which obtains here, as in *Kalanchoë*, that the full size of internode is reached sooner in the control than in the CO_2 plants.

The stem and leaves are armed with spines which were somewhat more numerous and very much better developed in the normal (control) plants.

The proportion existing between the size of the leaves in the air and CO_2 plants respectively is 1·0 : 0·6. As regards the anatomical differences these are in the direction such as might have been anticipated from a consideration of the diminished leaf surface. The xylem is less well developed in the experimental than in the control plants, whilst the phloem again shows but little variation. The hypodermal collenchyma is not so well marked in the CO_2 as in the air plant, exactly as is the case with *Kalanchoë*.

The mesophyll of the leaf was somewhat more spongy in the CO_2 plant than in the control, a fact perhaps to be correlated with the greater number of stomata in the former.

No difference was observed in the structure of the roots of the two series.

The number of stomata per unit area of leaf surface was greater in the CO_2 than in the air plant in the proportion of 1·0 : 1·3. The relative number of epidermal cells and stomata in the two series was 100 : 43 (air) and 100 : 48 (CO_2) respectively.

The cells of the leaves (including the guard cells) and of the wood parenchyma were gorged with starch in the case of the CO_2 plant, and the guard cells of the stomata were widely open as contrasted with their closed position in the control specimen.

Begonia gracilis.—The relation between the average lengths of the four youngest internodes in the two series is expressed by the ratio 1·0 : 0·5 for the air and CO_2 plant respectively. But the internodes of the latter do not reach their full length as soon as do those of the control plant: in this they resemble the foregoing species.

The proportionate size of the leaves in the two series is about 1·0 : 0·56.

The stomata in the leaves of this plant are aggregated in groups, these being separated from one another by large epidermal cells, that junction as water-storage elements. The leaves of the CO_2 plant contain a larger number of stomata per unit area than those of the control plant, the

proportion being 1·0 : 2·3 (fig. 3, A and B). But this large increase of the number in the case of the experimental plant is solely due to a diminished size of the water-storing epidermal cells, whereby a larger number of stomatal groups are included in a given area. There is no average increase of the number of the stomata within a group, and hence the disparity is clearly due to the effect of the additional carbon dioxide in arresting the growth of the ordinary epidermal cells before they have reached their full size. The guard cells are similar in size in the two series, and are not affected in the same way as the rest of the epidermis.

The anatomical structure of the stem and leaf presents no features of special interest beyond the character of the epidermis just described. The remaining differences were of a minor character, and of too little constancy to warrant any general points of distinction being traced between the two sets of plants.

No difference was observed between the root structure in the two series.

The cells of the leaf and of the ground tissue of the stem were densely filled with starch in the case of the CO_2 plant, but not strikingly so in the control specimen. Furthermore, the latter was rich in crystals of calcium oxalate, which were absent from the CO_2 plant. This is of some importance, as indicating that probably the lack of starch in the air plant is to be connected with the utilisation of the carbohydrate, which in the other is simply stored up in an insoluble form. A similar relation exists in the other species in which these crystals occur.

Impatiens platypetala.—The proportion between the sizes of the leaves in the air and CO_2 plants was found to be 1·0 : 0·76. Owing to the difficulty encountered in securing suitable preparations of the epidermis, no estimates were arrived at as to the comparative numbers of stomata.

As regards the internal anatomy, the relation between the relative development of the conducting elements of the xylem showed, as was to be expected, the same kind of difference as in the case of the plants already described. In the leaf, the CO_2 specimen had the advantage in thickness and in the size of the cells. The contrast was most striking in the palisade layers, which were distinctly double in the experimental plant, as compared with the slight elongation characterising the cells of the corresponding layer in the control plant (fig. 4, A and B). The intercellular spaces were also larger and more numerous in the former.

The cells of the CO_2 plant contained very large quantities of starch, as in the examples previously described, and crystals of calcium oxalate also occurred, though not so abundantly as in the tissues of the control plant.

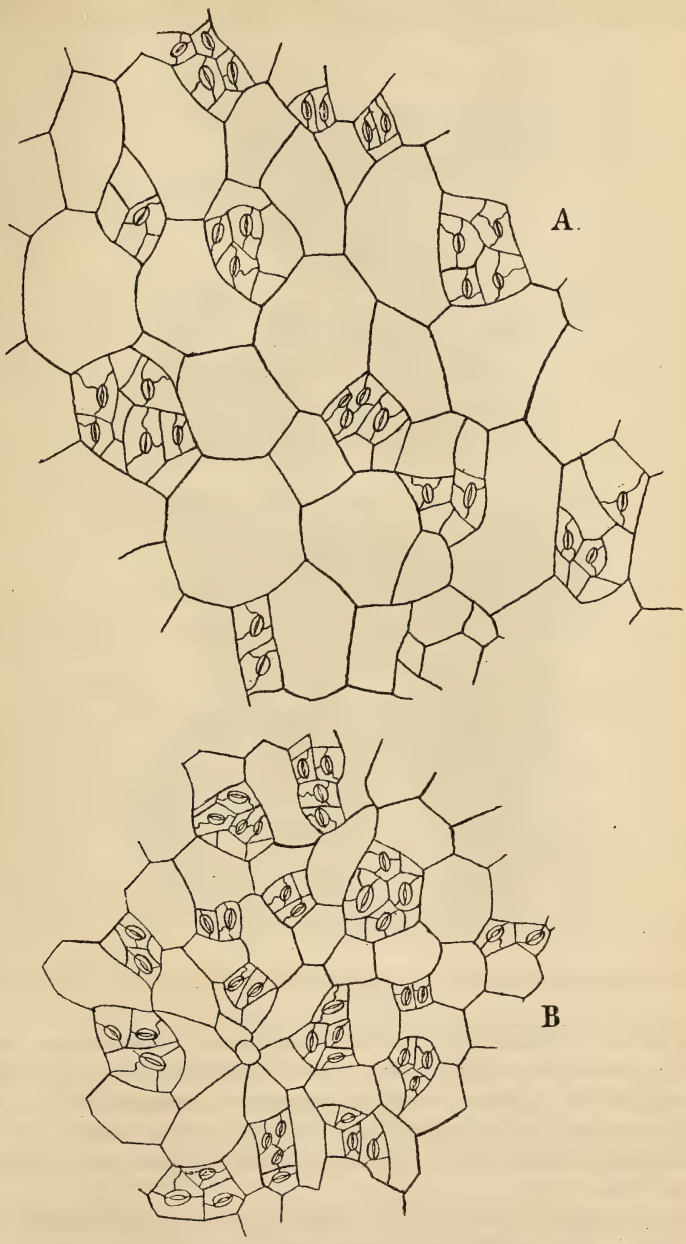


FIG. 3. *Begonia gracilis*, epidermis from lower surface of leaf. A, air- ; B, CO_2-plant.

Fuchsia, sp.—The remarkable appearances presented by the experimental plants have been described by Brown and Escombe in their

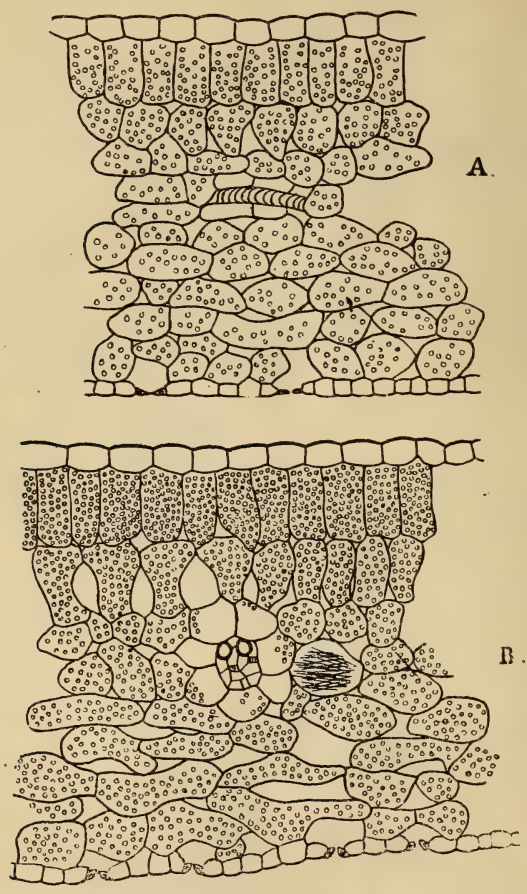


FIG. 4. *Impatiens platyphylla*, transverse section of leaf. A, air-; B, CO_2-plant.

paper already referred to. No very satisfactory measurements were made of these internodes with a view of ascertaining their relative lengths, but in a general way the appearance presented by the two series conformed to that already described for the other species.

The proportionate size of the leaves in the two series was estimated as 1·0 : 0·35.

The numerical relation of the stomata per unit area of leaf in the two cases is 1·0 : 1·4 (fig. 5, A and B), but the proportion of epidermal cells to stomata in the two series respectively proved to be almost identical, being 100 : 32 (air) and 100 : 31 (CO_2).

The only feature of anatomical difference in the stems of the two series consisted in the inferior differentiation of secondary wood in the CO_2 plant, whilst the phloem was equally well developed in both.

The roots were perfectly similar in the two series. Starch occurred in much larger quantity in the leaf, and ground parenchyma of the CO_2 plant than in the control example. Crystals of calcium oxalate occur in the leaves of both series, and appeared to be about equally distributed over equal areas.

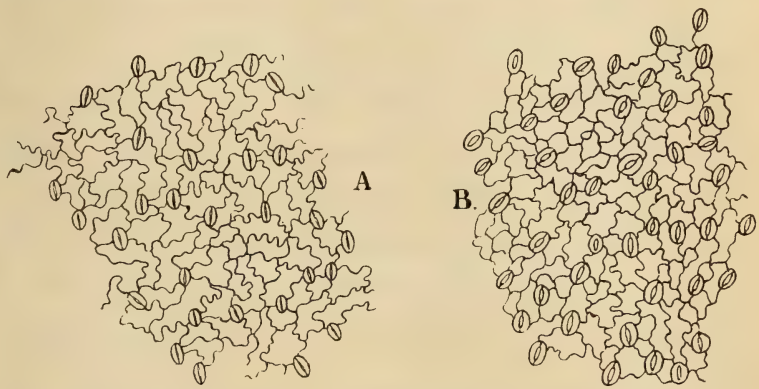


FIG. 5. *Fuchsia*, sp., epidermis from lower surface of leaf. A, air-; B, CO_2-plant.

The results upon the structure of flowering plants, consequent on increasing the amount of carbon dioxide normally present in the atmosphere to about three and a half times this amount (*i.e.*, when it is present in the proportion of about 1 in 1000 volumes) may be briefly summarised as follows:—

1. The growth of the internodes is checked, and the period of growth as measured by the elongation of successive internodes is lengthened.

2. The growth in surface of the leaves is arrested at a more or less early stage.

3. The absolute number of stomata *per unit area* of leaf surface is considerably increased, largely or entirely as the result of the non-attainment by the epidermal cells of their normal size. The guard cells of the stomata do not however share in this diminution, but are, if anything, larger in plants treated as above. The guard cells also are gorged with starch, and the stoma remains open even when the leaf is killed in spirit. The *relative* proportion of the stomata to the number of epidermal cells in a given area, remains approximately constant for both leaves treated with the additional carbon dioxide, and those which have been grown under normal conditions. But although the stomata show the increase in number just referred to, their total number on the whole surface of a treated leaf may be smaller, on

account of the great diminution in the size of the latter as compared with that of the plant grown under normal conditions, and because the total number of the whole epidermal cells in the smaller leaves may also be smaller than that of the epidermis of a normal leaf.

4. The anatomical structure of the internal tissue of the leaves is not materially altered, any modification that may arise consisting chiefly in the relative number of cell layers, and of the abundance of intercellular spaces formed.

5. The anatomical structure of the stem commonly differs in the direction of the formation of less lignified elements of the xylem, a smaller number of vessels, and frequently also in the imperfect development of the mechanical tissues. The phloem, on the other hand, shows no alteration. The change in the xylem is almost certainly related with the diminished leaf surface, and consequently reduced transpiration. But bearing in mind the stomatal relations mentioned under 3, it is probable that the reduction is also connected with disturbances of the metabolic processes which may act, not only in the way here indicated, but also may more directly affect the means of supplying material for the growth in thickness of the cell walls.

6. The last point is emphasised by the invariable accumulation (except in *Kalanchoë*, and it will be remembered that the metabolic processes of succulent plants are frequently peculiar) of starch in the leaves and ground parenchyma of the treated plants. As regards *Kalanchoë*, the guard cells of the stomata, which are in many respects comparatively isolated from the other tissues, do contain more starch in the treated than in the normal plants. It is also worth noting that in this plant the tannin so characteristic of the specimens grown under normal conditions is very much reduced in the experimental series, a fact which further points to a disturbance of the ordinary course of metabolism.

7. No alteration could be detected in the roots as a consequence of the increase in the carbon dioxide contents of the atmosphere.

8. When crystals of calcium oxalate occurred in a species they were always less abundant in the treated than in the control plants, with the possible exception of *Fuchsia*.

It remains to be stated that the results here described apparently differ in a remarkable way from those obtained by Téodoresco,* who investigated the action of increased carbon dioxide upon growth. The conditions of the experiment, however, in the two cases were by no means similar. Téodoresco compared plants which had been grown in an atmosphere entirely purified from carbon dioxide with others grown in air containing an amount varying from 1·5 per cent. to 2 per cent. of this gas. He found that under these conditions, the plants

* E. C. Téodoresco, "Influence de l'Acide Carbonique sur la Forme et la Structure des Plantes," 'Rev. Gén. de Botanique,' vol. 11, 1899.

treated with the excessive carbon dioxide grew more luxuriantly, and exhibited more complete internal differentiation than those deprived of this source of carbon.

This result is perhaps hardly surprising, as the one set of his plants was entirely deprived of its source of atmospheric carbon dioxide, and it would have been interesting to have compared his specimens at the end of the experiment with others grown in normal air, but otherwise under similar conditions of temperature, illumination, &c.

It is, however, worthy of notice that Téodoresco kept the atmosphere round his plants in a tolerably dry condition by means of sulphuric acid. This might tend to promote transpiration, and it may be that the apparent discrepancies between his plants and our material as regards both structure and histological differentiation is partly perhaps to be attributed to this circumstance. But only further investigations can settle this and many other points of interest connected with the influence, direct as well as indirect, of alteration in the constitution of the atmosphere on plant-structure.

"Preliminary Report on the Recent Eruption of the Soufrière in St. Vincent, and of a Visit to Mont Pelée, in Martinique." By TEMPEST ANDERSON, M.D., B.Sc., F.G.S., and JOHN S. FLETT, M.A., D.Sc., F.G.S. Communicated by the Secretaries of the Royal Society. Received August 11, 1902.

[PLATES 11–13.]

Dr. Tempest Anderson and Dr. John S. Flett, who received a commission from the Royal Society to investigate the recent volcanic eruptions in the West Indies, more especially in St. Vincent, submit the following preliminary report:—

We arrived at Barbados on June 8 (having left London on May 28), and thence proceeded to St. Vincent, where nearly 4 weeks were spent, mostly at Chateaubelair and Georgetown, in the vicinity of the Soufrière. On June 29, Dr. Tempest Anderson went to Grenada to examine the lagoon at St. George's, returning some days later, Dr. Flett remaining at St. Vincent to complete his investigations.

On July 6 we arrived at Martinique, and on the 12th left that island for Dominica, where we remained until July 17, when we returned to Barbados. In all 6 weeks were spent in the West Indies.

In the Windward Islands, in the month of July—the middle of the rainy season—the work of a geological expedition is necessarily attended with many difficulties, but these were greatly mitigated by the kindness rendered by all with whom we came in contact. To Sir Robert Llewellyn, K.C.M.G., the Governor of the Windward Islands, and to the Administrators of St. Vincent, St. Lucia, and

Dominica—and other officials connected with the Colonial Office—we are especially indebted for information, advice, and assistance. Dr. Morris, C.M.G., of the Imperial Department of Agriculture for the West Indies, and the members of this department in the various islands, received us with the greatest kindness, and gave us invaluable help throughout. The many planters and overseers of estates, medical men, and merchants to whom we made application for information or for assistance, received us with that courtesy and hospitality which is characteristic of the Colony, and did everything in their power to forward our work in every way. In Martinique, the Governor of the island gave us every facility for conducting our scientific investigations. In this brief preliminary report it is impossible for us to mention by name even a small proportion of those who, often at considerable inconvenience to themselves, lightened our labours by their kind provision and forethought. In every place we visited we found friends who were willing to direct, assist, and accompany us; and without their help it would often have been impossible for us to make satisfactory arrangements or to accomplish our work. In this way what might have been a very arduous undertaking was greatly lightened, and we wish to place on record our deep indebtedness to our many kind friends in the Windward and Leeward Islands.

The island of St. Vincent is of oval form, 18 miles by 11 miles, the longer diameter being nearly north and south. A mountain chain stretches along the main axis of the island and reaches to a height of 2000 to 4000 feet—the highest point being just over 4000. It is entirely composed of volcanic materials, the beds of lava and tuff dipping away in all directions from the central mass towards the sea. In the southern part of the island, volcanic action has long been extinct or dormant, but at the northern end stands the still active Soufrière—a striking volcanic cone 4048 feet in height, with a crater nearly circular in form, and about 1 mile in diameter. Along the leeward side of the island very fine sections are exposed, and these show it to consist of volcanic rocks of which by far the commonest is a coarse andesitic agglomerate or tuff, though there are many lava streams which may be traced as vertical cliffs along the valley sides alternating with the gentler slopes, due to the thick beds of ash. The scenery is bold and picturesque, the heavy tropical rains acting on steep slopes having effected rapid and intense erosion. As a consequence, deep valleys radiate out from the central ridge, separated by high narrow spurs running down to the sea. On the windward side below the level of 700 feet above the sea there are considerable stretches of flat or gently sloping ground, on which stand many of the most important arrowroot and sugar estates. The first glance at these lower grounds suffices to show that they are very distinct in configuration from the highly eroded and deeply sculptured uplands,

and further investigation reveals the presence of more or less well-marked beaches or terraces—a system of old sea beaches or rock platforms partly obliterated by subaerial erosion, and in some places covered with *débris*. On both sides the submarine slopes are steep, but most so on the leeward coast; as on the east or windward side there is a considerable expanse of shallow water in which a submerged terrace at a depth of 150 feet can be traced by means of soundings, as indicated on the charts. There are no raised coral beaches here as in some of the other islands, and the latest movements of the land have probably been in a downward direction.

The Soufrière mountain forms the northern extremity of the island, and its general form at once suggests a comparison with Vesuvius. It is a simple cone without lateral or parasitic craters. The one at its summit is surrounded on the north side by the remains of a gigantic crater ring, which has the same relation to the present crater as Somma has to Vesuvius. On the north-east lip of the main crater there is a smaller one known as the New Crater, as it is believed to have originated in the eruption of 1812. It is only one-third of a mile in diameter. It is doubtful whether the New Crater was active during the late eruption, and there can be no doubt that it was from the principal or "Old Crater," that the materials mostly were emitted. Deep valleys, often with precipitous sides, have been cut in the slopes of the mountain, especially on its southern side, and it is in these—and particularly in the Wallibu, Rozeau, and Rabaca Dry River—that the greater part of the ejecta of the recent eruption have collected.

The eruption of May, 1902, though sudden in its outburst and disastrous in its effects, was far from unexpected. In the north of St. Vincent there were two settlements of the aboriginal Caribs, and these had been so startled by the frequent violent earthquakes, that in February of last year they were considering the advisability of deserting the district. But the first signs of actual volcanic activity were on Tuesday, May 6. The inhabitants of the leeward side were fortunate in having a clear view of the crater, and warned by the outbursts of steam they fled to Chateaubelair, and other places along the coast-line to the south, so that few lives were lost in this quarter. But, on the windward side, the summit of the mountain, as is frequently the case, was wrapped in cloud. Here, at the base of the mountain, there is an extensive stretch of flat land, known as the Carib country, on which were situated some of the largest and richest estates in the island, with a dense population mostly black or coloured. So little alarm was felt here, that even on the morning of Wednesday, May 7, when the leeward side was practically deserted, sugar-making was in progress on several estates, and all the operations of tropical agriculture were being conducted as usual. From Kingstown, telephonic messages

were sent to Georgetown, which is not far from the base of the hill, stating that the Soufrière was in eruption, but they appear to have occasioned little anxiety. And when, about mid-day on Wednesday, the danger was too obvious to be overlooked, the Rabaca Dry River, and some of the streams on the windward side, usually dry except after rains, were running boiling hot, and could not be crossed. Many fugitives in this way found their escape cut off. It was here that the loss of life was greatest, which, though many escaped, is estimated to have amounted to 2000, including about a dozen white men—the overseers of the plantations. The exact number will never be known, as many were entombed in the ashes where they fell.

About mid-day on Tuesday the first signs of the eruption were observed by those dwelling on the south-western side of the mountain. At 2.40 that afternoon there was a considerable explosion, and a large cloud of steam ascended into the air. By 5 o'clock a red glare was visible in the steam cloud on the summit. Activity continued during the evening, and at midnight there was a great outburst, and red flames were noticed on the lip of the crater. Next morning from Chateaubelair a splendid view could be obtained of gigantic mushroom-shaped clouds rising to a great height in the air—estimated at 30,000 feet—and drifting away before the north-east trade wind. As the day advanced the eruption increased in violence; by 10.30 A.M. enormous clouds of vapour were being emitted with loud noises, accompanied by much lightning. It is remarkable that at that time the inhabitants of the windward side were still in doubt about the reality of the eruption, since they mistook the dark cloud covering the mountain for a thunder cloud. The mountain was now in a state of continuous activity, and from Chateaubelair it could be seen that the materials were mostly discharged from the old or principal crater. Vast clouds of steam, showers of dark matter (probably mud), and of stones, could be seen projected from it, partly on the leeward, but mostly on the windward side. At mid-day the slopes of the mountain were still green, and the rich mantle of tropical vegetation had not yet been destroyed. A thin layer of fine ash had fallen over the lower ground, only sufficient to give the leaves a greyish colour. The enormous columns of vapour continued to ascend from the crater, with frequent violent outbursts, projecting showers of stones and mud.

About this time it was noticed that steam was rising from some of the valleys on the south side of the hill, and this increased till at 12.50 the whole mountain was suddenly enveloped in a dense cloud of vapour. Just before this the Rivers Wallibu and Rabaca had been seen rushing down in raging floods of boiling water. It is most probable that these phenomena were due to the escape of the crater lake which was driven over the lower or south lip of the crater between

12 o'clock and 1 o'clock on the Wednesday afternoon, and poured down the valleys to the sea. So far as we know there were no mud lavas, in the ordinary sense, flowing down these valleys, but only a tremendous rush of boiling water, which left no traces which we could recognise when we visited the district.

By 1 o'clock the roaring of the volcano was tremendous. Showers of stones were being projected both to windward and to leeward. The enormous columns of steam continued to ascend from the crater. The lightnings were terrific, and after the large outbursts, which took place every few minutes, volumes of vapour might be seen covering the whole area. Hitherto the eruption had been of a type with which geologists are familiar, and the destruction done was confined to the higher parts of the mountain in the close vicinity of the crater.

But about 2 o'clock—to quote the words of an eye-witness (Mr. T. M. McDonald, of Richmond Vale Estate)—"there was a rumbling and a large black outburst with showers of stones, all to windward, and enormously increased activity over the whole area. A terrific huge reddish and purplish curtain advanced to and over Richmond Estate." This was the strange black cloud which, laden with hot dust, swept with terrific velocity down the mountain-side burying the country in hot sand, suffocating and burning all living creatures in its path, and devouring the rich vegetation of the hill with one burning blast.

On the leeward coast few were overtaken by the black cloud, as the inhabitants had fled and taken refuge in the villages south of Chateaubelair. Those who were caught were killed or badly burned. One boat was near Richmond at the time the blast swept down. They describe the heat as fearful. Hot sand rained into the boat, and the sea around was hissing with its heat. The darkness was so complete that a man could not see his hand. They saved their lives by diving into the water; when they returned to the surface the air was suffocating, but they continued to dive again and again, and, when at their last gasp, they found that the air cleared, and they could breathe again. This occupied only a few minutes—probably much less in reality than it appeared to them. One man was too exhausted to continue diving; he clung to the gunwale of the boat, and the tops of his ears were severely scorched.

It may be worth while to quote the descriptions of a few spectators who saw this cloud from a safe distance. Dr. Christian Branch, of Kingstown, writes: "We saw a solid black wall of smoke falling into the sea about 2 or 3 miles from us. It looked like a promontory of solid land, but it rolled and tumbled and spread itself out until in a little time it extended quite 8 miles over the sea to the west . . . . Then began the most gorgeous display of lightning one could conceive . . . . It was still bright daylight, but the whole atmosphere quivered and thundered with wavy lines intersecting one

another like trellis-work. We were encircled in a ring of fiery bayonets."

Another eye-witness (the Rev. Mr. Darrell, of Kingstown), who was in the same boat with Dr. Branch, describes it as follows:—"We were rapidly proceeding to our point of observation when we saw an immense cloud—dark, dense, and apparently thick with volcanic material descending over our pathway, impeding our progress and warning us to proceed no further. This gigantic bank of sulphurous vapour and smoke assumed at one time the shape of a gigantic promontory, then as a collection of twisting, revolving cloud whirls, turning with rapid velocity—now assuming the shape of gigantic cauliflowers, then efflorescing into beautiful flower-shapes, some dark, some effulgent, some bronze, others pearly white, and all brilliantly illumined by electric flashes."

On the windward side of the island an uninterrupted view of the progress of the eruption could not be obtained, owing to the veil of cloud which obscured the summit. By mid-day on Wednesday even the most sceptical were convinced that the Soufrière was in eruption, and that the noises heard continuously were not due to a thunderstorm. Before mid-day there had been very heavy rain-showers, and it was noticed that the rain-drops carried down fine particles of ash. Work ceased on the plantations, and those labourers who still remained endeavoured to escape to Georgetown or shut themselves up in their houses. By 2 o'clock fine ashes, with occasional larger stones, were falling steadily, but, as yet, little damage had been done, and no one had been injured. Then came the climax of the eruption, and those who were in the open air saw a dense black cloud rolling with terrific velocity down the mountain. They took refuge in their houses and in the plantation works, where they crowded together in such numbers that in one small room eighty-seven were killed. The cloud was seen to roll down upon the sea, and was described to us as flashing with lightning, especially when it touched the water. All state that it was intensely hot, smelt strongly of sulphur, and was suffocating. They felt as if something was compressing their throats, and as if there was no air to breathe. There was no fire in the ordinary sense of the word, only the air was itself intensely hot and was charged with hot dust. The suffocating cloud only lasted a few minutes. Those who survived this ordeal mostly escaped, though many died within a few hours from shock, or from the severity of their injuries. In some cases a few survived, entirely or almost entirely uninjured, in a room in which many others died. Most of those who escaped had shut themselves up in the rum cellars or in substantially built houses, and had firmly closed all doors and windows. By the time the hot blast had reached the coast the sand it contained was no longer incandescent, and though still at a very high temperature it did not set fire to wood or

burn the clothes of those exposed to it. The burns on the survivors were chiefly on the outer aspect of the arms and legs, and on the faces, and confined to parts not protected by their clothes.

Complete darkness now covered the whole north-end of St. Vincent —a darkness more intense than any that the inhabitants had ever before experienced. The fugitives had to creep along the roads or feel their way along the roadsides. The roaring of the mountain was terrible—a long, drawn-out, continuous sound resembling the roar of a gigantic animal in great pain. Fine ash and sand rained down over the whole country with occasional showers of large stones. Some of these were so hot as to set fire to the "trash" roofs of huts in the south-end of Georgetown, at a distance of 7 miles from the crater. In Kingstown, 12 miles from the Soufrière, the ash was at first moist but afterwards dry. It had a stroug sulphurous smell, and pattered on the roofs like a heavy shower of tropical rain. Around the volcano the earth shook and trembled continuously, and the motion was described to us as undulating rather than resembling the sharp shock of an earthquake. Only in one or two cases were the walls of houses injured. What was taking place on the summit of the mountain no one can tell, but all who passed that night in the vicinity of the Soufrière agree that there was one black suffocating cloud and only one. In all probability the eruption had reassumed the ordinary phase, and the showers of ash and stones were produced by violent upward explosions of steam. By half-past 5 o'clock the ash was falling in Barbados, 100 miles to the eastward, whither it had been carried by the upper currents of air in a direction opposite to that of the trade winds. In St. Vincent the darkness lessened slightly before nightfall, but the rain of dust and the noises lasted till early in the ensuing morning.

When day broke it was seen that in St. Vincent, and even in Barbados, everything was covered with fine grey ash, resembling a fall of snow. The dust had penetrated into the interior of the houses, where it lay in a thin film on walls and furniture. In Kingstown there were stones as large as a hen's egg; in Georgetown and Chateaubelair some had fallen as much as 1 foot in diameter. Little damage, however, appears to have been done to growing crops, except in the north end of the island. In fact, many believe that the sulphurous ash had insecticidal properties, and benefited the vegetation. From Chateaubelair it could be seen that the volcano was still emitting puffs of slaty coloured steam, and showers of fine dust were falling on the leeward side of the mountain. For several days these discharges of vapours continued, but a new phenomenon now attracted more attention. The ravines which furrow the south side of the mountain were found to be discharging clouds of vapour, and this gave rise to reports of fissures having opened on the flanks of the Soufrière, of subsidiary eruptions arising from these fissures, and of streams of lava flowing

down the valleys. As a matter of fact, they were really due to the action of water flowing through the hot sand, which in some places had almost obliterated the old stream courses, as will be explained more fully later on. By the 15th the volcanic activity had apparently subsided, and the mountain remained clear and unclouded. The explosions of steam in the valleys continued, and are probably still going on.

The state of quiescence continued till Sunday, May 18. Confidence was being restored, and the inhabitants of those districts near the mountain which had not suffered severely were returning to their homes. On the windward side, the work of burying the bodies had been completed and things were resuming their normal course. But about 8 o'clock that evening an ominous sound was heard from the crater. Its nature was at once recognised and struck the black population with terror. The noises were as loud as those of the first eruption, and the lightning was very vivid. On the leeward side complete darkness prevailed, and ashes and sand fell freely for some hours. In Georgetown the fall of ashes was quite inconsiderable, not exceeding a thin film on the roof of the houses. Gradually the noises lessened, the darkness lifted, and the moon appeared again. No lives were lost and practically no damage was done, but exactly what happened on those parts of the mountain nearest the crater it is, under the circumstances, impossible to say. This second eruption was the last which proceeded from the main crater. Clouds of steam were sometimes seen gently rising for some days later, but nothing of the nature of a volcanic outburst has since taken place.

We arrived at Kingstown on Tuesday, June 10, and proceeded at once to Chateaubelair, where Mr. Jas. E. Richards, of Kingstown, kindly placed a house at our disposal. The geological products of this eruption proved to be of very simple character. The Soufrière and the surrounding country were covered with a layer of ashes mostly in the form of fine dark coloured sand, but mixed with spongy bombs of various sizes and many ejected blocks composed of fragments of the old rocks of the hill. Lapilli and scoria are there in plenty, as is obvious where the heavy rains have washed away the finer material, but the greater part of the ejecta consist of fine sand which, when dry, is hot and yellowish-grey in colour, but when wet becomes almost black. This sand, as has already been noted by many observers, contains plagioclase felspar, hypersthene, augite, magnetite, and fragments of glass, and represents a fairly well crystallised hypersthene-andesite magma which has been blown to powder by the expansion of occluded steam.

The coarser material is mostly a slaggy andesite with crystals of plagioclase and pyroxene. There is little pumice, though we obtained a few fragments which floated on water and contained but few crystals visible to the naked eye. The larger bombs are often black, highly

lustrous and glassy when broken across. Some were seen at Wallibu (4 miles from the crater) 3 feet in diameter. The ejected blocks consist of weathered andesites and andesitic tuffs such as can be seen in the walls of the crater. They are very numerous, and some are over 5 feet across. In addition to these, fine-grained dark-green banded rocks occur, which appear to be baked and indurated sediments, probably the mud from the bottom of the crater lake, or the finer beds intercalated in the older volcanic series. Another type of ejected block which is very common in some parts of the hill is a coarse-grained aggregate of felspar, hornblende (brown under the microscope), and perhaps olivine. It is not vesicular and contains little or no glass, being apparently holocrystalline. These rocks are very friable, and the crystals are loosely aggregated together. They seemed to us to be comparable to the sanidinites of the Eifel and many other modern volcanic districts. They are certainly quite unlike true plutonic diorites, both in their structure and in the character of their minerals.

It may be noted that none of these rocks are characteristic of this eruption, but all can be found among the older materials of the hill. The hardened, baked sediments were well known to the Caribs, who have long used them for the manufacture of their finer stone implements. The felspar-hornblende blocks were found by us among the older rocks, and in some places even as rounded masses enveloped in the old lavas. Some of the fresher bombs in the river beds and on the seashore can hardly be distinguished from those which were the product of this eruption, though undoubtedly of much older date.

At Kingstown, as in Barbados, the deposit of volcanic dust and sand was so slight that, owing to the heavy tropical rains, and the rapid growth of tropical vegetation, it readily disappeared, and when we arrived it was necessary to make careful search to find traces of it. In St. Vincent, to the south of Chateaubelair, on the leeward side, and from 2 miles south of Georgetown, on the windward side, the country had very much its normal appearance. To the north of these points, however, a sheet of volcanic ejecta covered the ground. Where it was thin it was rapidly disappearing. Every shower washed much of the finer matter into the streams, which were flowing full of sand and lapilli to the sea. In the fields the arrowroot was pushing up through the layer of ash, and covering it with a mantle of green leaves. Around Georgetown the deposit is from 1 to 3 feet deep, and some of the blocks are a foot in diameter. On some of the sugar-cane fields in the Carib country the ash lies 4 feet deep, while on the higher slopes of the hill it is from 5 feet to over 12 feet (where it has gathered in the hollows). On the leeward side the ash is very deep in the valleys of the Wallibu and Rozeau Dry Rivers, but north of Larikai it is much thinner, not above a foot or two. The north side of the

mountain has, for reasons to be subsequently discussed, received comparatively little of the deposit, and at Point Espagnol, Owia, Fancy and Quashie Point, along the north shore, the cliffs and the country for some short distance behind them are perfectly green and flourishing.

On the south side of the Soufrière a deep and broad valley has been eroded in the soft volcanic ash and agglomerate, of which this part of the hill consists. It runs almost across the island, between the Morne Garu Mountain and the Soufrière, and it is this valley which has received the greater part of the ejecta of this eruption. The streams which flow into it—the Wallibu River on the west and the Rabaca Dry River on the east—have had their courses filled with fine hot sand mixed with coarse bombs and ejected blocks. We were told that on the west side the ravine of the Rabaca Dry River had been about 200 feet deep. It is now almost entirely filled up, and the river is slowly cutting its way through the hot sand which occupies it. The same thing is happening in the Wallibu Valley, but here erosion is more advanced, and cliffs of grey hot ash, some 80 feet high, overlook the stream at a point about a mile above its mouth. On the flatter ground between the river gorges which trench these broad valley bottoms the deposit is very much thinner, perhaps 3 to 5 feet on the windward side, but often 12 feet, and sometimes 30 or 40 feet, on the leeward side.

The distribution and thickness of the recent ashes is not at all such as would have been expected had these materials merely rained down from above. Wherever there is a hollow it has been filled up, however deep. For some days after the eruption the stream valleys were level with their banks. On the flat ground the deposit is much thinner, and on the ridges and spurs which stand up prominently there was comparatively little accumulation. To the mind of a geologist examining these valleys one comparison was irresistibly suggested—they resembled nothing so much as a rugged country covered with blown snow. The ash had drifted into and filled up the depressions, while comparatively little had rested on the ridges between. It is conceivable that mud lavas flowed down at an early period in the eruption, and occupied the lower parts of the gorges; but we saw no evidence of this, and as wherever the deeper layers of the ash are exposed they are still burning hot, it is obvious that they could never have reached their present position in the condition of a mud lava. When we saw this country its surface had been deeply scored by the rains, but those who visited it shortly after the first eruption described it as having a smooth, gently rolling surface like that of blown sand. This is well shown in photographs taken by Mr. Wilson, of Kingstown, on May 14. The conclusion was forced upon our minds that immense quantities of hot sand had rushed down the hill into these valleys in an avalanche

which carried with it a terrific blast, and piled the ashes deep in the sheltered ravines, at the same time sweeping everything off the exposed ridges which lay between. The rain of volcanic material, which lasted for hours after the hot blast had passed, then covered the surface of the country with a final sheeting of fine dust and scoria.

When we ascended the Soufrière, the evidence of the passage of a hot blast laden with sand was overwhelmingly clear. The various stages of its action, and its varying intensity at different spots, are most easily observed on the windward side, where the country is more flat and open, and there are fewer ravines and spurs to modify the course of its operations than in the Wallibu Valley.

The track to the summit passes across the Rabaca Dry Valley near the shore, then turns upwards through the sugar-cane fields of Rabaca and Lot 14. These were covered with 3 or 4 feet of sand and scoria, the trees all bare, their leaves stripped by the falling cinders; but few branches were broken, and no trees had been uprooted or cast down. The woodwork of the houses was unburnt, though the roofs of some of the verandahs, and of the labourers' huts, had collapsed from the weight of ashes that had fallen on them. Many people were killed on these estates. The survivors described to us how the dark cloud had rolled down from the mountain, and how hot and suffocating the air had been when it enveloped them. But it was evident that the velocity of the blast was not above that of an ordinary gale, and the dust it carried, though hot, was not incandescent.

At Lot 14 it was seen that many trees had their limbs twisted off and broken, and some of the negroes' houses had taken fire (probably mostly from hot falling bombs). The blast was more violent here, but not hot enough to set fire to the woodwork or char the green wood of the standing timber.

On the flat ground above the plantation buildings (at an elevation of about 1000 feet), a further stage of devastation was encountered. The fields were here swept bare, the trees broken down though not as a rule uprooted, their smaller branches swept away; a deep layer of black sand covered the crops of sugar-cane. The blast was here a violent gale.

A little further up the effects of the blast were remarkable. Enormous trees had been uprooted and cast down. Their leaves and finer branches, of course, had disappeared. In every case the fallen trunks pointed directly away from the crater. Even the great cotton-trees, 10 feet or more in diameter, were broken off or uprooted. The smaller trees had in a few cases been swept away like straws. The larger were merely cast down, and lay side by side, their tops directed down the valley, their roots towards the summit of the mountain. Most were charred, some deeply, but, as the wood was green, only the smaller branches had been consumed. The effect was like that pro-

duced by a violent hurricane, only more complete, for many of these trees had withstood the hurricane which ruined St. Vincent in 1898. At the lower limit of this region some curious effects of the hot sand blast could be seen. Where any branches or trunks were still standing, they invariably showed themselves to be burnt and eroded on one side—that next the crater—the wood having been charred and the charred material removed by the action of a hot sand blast. On the side away from the crater, the original bark was still left, unburnt but dry and peeling off; that is, there had been no erosion on the sheltered or lee side of the stems. The wood was too green to take fire, but the sand had been sufficiently hot to char the surfaces which were exposed to it.

Further up the hill—that is to say, above the 1500 feet level, there was little left of the rich tropical vegetation which had covered it from summit to base. Blackened remains of tree-trunks were to be seen, overturned or broken off near the ground, and buried in dark sand. The highest parts of the mountain are as bare and desolate a scene as could be imagined. The ash is 5 to 12 feet deep, and though full of large blocks and spongy bombs, is mostly so fine that when thoroughly wet it becomes a mud, very tenacious and slippery, in which one sinks to the knee. In it there is a good deal of burnt timber, utterly blackened and converted into charcoal. Everything has been mown down, and at the same time the intense heat has consumed all the smaller fragments and charred the larger. There is nothing to show what was the velocity of the blast when it left the crater. After a couple of miles it was that of a hurricane or tornado. The limits between the zone of uprooted trees and that of trees still standing, but broken and much damaged, is surprisingly sharp. At 4 miles from the crater the blast was travelling at 20 to 40 miles an hour, and rapidly slowing down. This agrees with the evidence of an eye-witness who saw it when it reached the sea near Chateaubelair. It came over the water with a wave before it, but it did not overturn the small boats which lay in its course.

Another peculiar feature of this blast is the manner in which its course was modified by irregularities in the configuration of the ground over which it passed. To the north of the crater stands the encircling crater wall, already referred to as the Somma. There can be no doubt that a black cloud descended over this side of the mountain, though here the devastation is comparatively slight, and it is inferred that the high intervening ridge overlooking the crater served as a rampart and helped to protect the country behind it from the effects of the blast. The southern lip of the crater, on the other hand, is the lower, and the avalanche of hot sand seems to have poured over this lip almost like a fluid. Down the deep open valley between the Soufrière and the Morne Garu Mountain it rushed, ever

following the steepest descent. It clung to the valley bottoms and coursed along them in a manner which somewhat recalls a raging torrent in a river. The streams in these valleys after descending the first part of the hill turn sharply at a right angle towards the coast, deflected by the opposing mass of the Morne Garu. The hot blast mostly followed these valleys, and in them it piled up enormous deposits of sand, but part of it swept up the shoulders of Morne Garu, and tore up the heavy timber which was growing there. The direction in which the fallen trunks point shows that the blast was split into two parts—one taking the east and one the west side of the mountain, rushing upwards obliquely from below. The mountain protected the country behind, and the line of demarcation between the burnt and the green forest almost corresponds with the dividing ridge. The south side is green; the north side towards the Soufrière is devastated and burnt.

The effect of even comparatively small ridges in deflecting the blast and protecting the country behind them is still more noticeable near Chateaubelair. Between the Rivers Wallibu and Richmond there is a high dividing ridge. The northern valley (the Wallibu) is filled with ash and utterly burnt up, that to the south (Richmond Valley) is in large part green. One side of the dividing ridge is blasted; on the other the arrowroot is again putting out its green leaves. Another ridge separates Richmond Valley from Chateaubelair. This ridge has been in many places scorched, but the country behind it has been perfectly protected, and, though covered with the rain of ash, has resumed its normal appearance. There can be little doubt these ridges served to direct the path and intercept the violence of the hot blast.

For some days after the eruption no rain fell, and the first to visit the district were able to observe the effects of the eruption unmodified by the erosive action of running water. But on May 25, $5\frac{1}{4}$ inches of rain fell. On the previous day the rainfall had been $2\frac{1}{2}$ inches, and the rainy season now set in in earnest. The effect of these deluges acting on loose material lying on steep slopes was phenomenal, and by the time we reached the island the surface of the sheets of ash had been sculptured into innumerable furrows and runnels. They cut down through the incoherent sand to the layer of burnt vegetation on the old soil beneath, or even into this, forming new channels, which varied from a few inches to many feet in depth. To one fresh from a temperate climate and unaccustomed to the power of tropical rains the rapidity of denudation under these conditions was astounding. On the upper part of the Soufrière beautiful feather patterns of rain rills converging towards a central main axis everywhere characterised the surface. The knife edges between the valleys were the only parts retaining the original smooth surface, and they formed excellent paths, as the sand was firm, except near the summit of the hill.

On the windward side of St. Vincent so much material is being swept into the sea by the streams that the coast is covered with black sand, and near Overland Village it is possible to walk for a mile beneath the sea cliffs on a broad, sandy beach, where formerly the heavy surf of a weather shore beat against their base. In the arrowroot fields the original surface is often to a large extent uncovered, and on the upper slopes of the Soufrière there are many places where none of the new ash is left, but the bare surface of the old rocks is everywhere exposed to view. After a heavy tropical shower, valleys which are usually dry may be filled with a thundering torrent several feet deep, and 20 or 30 feet across. Under these circumstances it will easily be understood that already many of the streams have thoroughly cleaned out the ash from the upper parts of their channels where the gradient is steepest.

But when such a torrent reaches the lower valleys, which have been filled with thick masses of hot sand, a strange conflict between fire and water can be witnessed. The river ploughs its way deeper and deeper, constantly sweeping the material into the sea. The valleys, at first almost obliterated, are now reassuming their old appearance. Terraces on their sides give evidence of former levels at which the streams flowed. There are five or six such terraces on the Wallibu. This river flows in gushes of hot steaming black mud, its intermittent flow being due to small land-slides temporarily damming up its channel, only to be swept away as the pressure of the water increases. On the Rabaca Dry River there has been less erosion, and only after heavy rains does it reach the sea, as the water from the smaller showers is apparently evaporated in its passage through the banks of hot ashes. After rains, both rivers can be seen steaming all along the lower parts of their courses.

When one of these streams comes down in force it undermines its banks by washing out the soft new ashes at their base. Then land-slides take place, and a curious spectacle results. When the hot ash tumbles down into the water, an immense cloud of steam rises in the air to heights of hundreds of feet. It expands in great globular masses exactly like the steam explosions from a crater, and as it drifts away before the wind fine dust rains from the cloud. We had the good fortune to witness a magnificent series of these explosions, one day as we were descending from the summit of the Soufrière. It was in the valley of the Rozeau Dry River. After every land-slip a column of muddy water rose to about 200 feet carrying with it pieces of stone. Immense quantities of steam shot up to 700 or 800 feet in the air. It resembled an enormous geyser of black mud and steam. In the Wallibu River, after every shower, these steam explosions may be witnessed taking place on a large scale. After a few hours of dry weather they cease, though the river can still be seen to be steaming strongly as it flows along.

The structural modifications produced upon the hill by this eruption have been astonishingly slight. We saw no fissures, no parasitic craters or cones, and no lava streams. Even the craters at the summit retain essentially their old configuration. All the evidence points to the supposition that it was from the large or old crater that this eruption for the most part proceeded. But the smaller crater has not disappeared, nor has it been filled up. We did not see it, but we can rely on the evidence of several observers, who knew it well before the eruption, and have seen it since. The narrow ridge between it and the large crater still stands, though probably somewhat lower than before, and possibly is slipping down in land-slides on both sides.

Like all the higher mountains of the Windward Islands the Soufrière has usually its summit capped with cloud, especially during the rainy season, and this was the case on both the days on which we made the ascent. On the first occasion the mist lifted for a few minutes, and enabled us to obtain a glimpse of the bottom of the crater. Fortunately we had with us Mr. T. M. McDonald, of Richmond Vale, and Mr. Henry Powell, Curator of the Botanic Gardens at Kingstown, who were both well acquainted with the mountain in previous years. The crater was formerly nine-tenths of a mile across and about 1100 feet deep. Its inner slopes were steep and richly wooded. Its bottom was occupied by a lake, which is said to have been over 500 feet deep. The northern wall is now a naked precipice of rock, perhaps 2000 feet high, from the face of which rock-slides are frequently tumbling into the abyss below, with a loud noise. We did not get a clear view of it, but Professor Jaggar, of Harvard, U.S.A., who ascended shortly before we did, was more fortunate, and obtained some photographs which show that it consists of layers of tuff alternating with beds of lava. What seems to be a thin irregular dyke forms a prominent rib-like mass cutting across the bedding planes. The southern side slopes downwards for several hundred feet at an angle of about 40 degrees, and is covered with a thick layer of fine dark mud deeply grooved with rain channels. The lower part is a precipice of bare rock. The bottom of the crater is nearly flat or slightly cupped. When we saw it, it contained three small lakes of water, greenish and turbid; that in the south-east corner was throwing up jets of mud and steam with a hissing noise. It was in very much the same condition as when seen by the party which first ascended the mountain on May 31—that including Mr. T. M. McDonald and Professor Jaggar—and on a slightly later date by Lieutenant Robinson, R.E. Mr. McDonald thought that there was rather less steam, and the lakes of water were somewhat larger than when he saw it previously. In his opinion and that of Mr. Powell the crater was only slightly larger than before the eruption, but considerably deeper. The estimates of the depth varied a good deal, but it seems, on the whole, to be generally agreed that it is about 1600 feet.

Accurate measurements of the breadth or depth of the crater were, under the circumstances, impossible. As seen from Chateaubelair the outline of the lip of the crater has suffered many modifications, though none of these is of any great importance. It is agreed that the southern edge is now somewhat lower than it was before the eruption, and this is confirmed by our barometric measurements.

It is reported that since we left St. Vincent the amount of water in the crater has increased, and, should this continue, a lake will ultimately be formed not unlike that which previously existed there. When the cliffs which form the north wall have reached, by repeated rock-falls, a condition of adjustment and stability, and when vegetation has again covered the interior slopes, it is possible that the crater of the Soufrière will have regained very much of its old appearance. Should any one who knew it before then return to visit it, he will have difficulty in believing that it formed the orifice from which were emitted the tremendous explosions of May 7, 1902. It was as we saw it an impressive spectacle, its naked rugged walls of rock looking down on the steaming lakes below.

Apart from the changes which have taken place within the crater, and the deposits of ash which have formed in the river valleys, and on the surface of the hill, the only other important geological modification of the country has been the disappearance of a narrow strip of coast along the leeward side of the island. Near the mouth of the Wallibu and from thence northward to Morne Ronde, the sea has encroached on the land for perhaps 200 yards. Below Wallibu plantation there stood a village of labourers' huts on a low flat beach with a bluff behind. Here the sea now washes the foot of a cliff some 30 feet high. This cliff consists of soft tuffs covered with several feet of new hot ashes, and is in an unstable condition, as masses are constantly falling down from its face. In this way a new beach is now forming in front of it. It is agreed by those who knew the district before the eruption that not only has the old beach disappeared, which carried the village and the public road, but that part of the bluff behind has also subsided. We were informed by Mr. T. M. McDonald, who is intimately acquainted with this coast-line, that similar subsidences had also taken place, though on a much smaller scale, at several places further north. There is no evidence elsewhere of any changes of level of land and sea. The tide-marks on the rocks and the landing-stages at the villages enabled us to ascertain that the level of high-water was at any rate within a few inches of what it had been before. It was clear that the alterations in the coast line were due to local subsidence of the foreshores, and that they had mostly affected loose and ill-consolidated deposits, such as beach gravels and the fans of alluvium which had formed at the mouths of the streams.

The submarine slopes on the leeward side of St. Vincent are

very steep, averaging about 1 in 4. Often within half a mile of the shore, or sometimes even less, the depth is over 100 fathoms.

It seems most probable that owing to the concussions and earthquakes produced by the explosions, some of the less coherent accumulations on these steep slopes slipped bodily into the deep. On this supposition most of the facts would be explained, but at the same time it is possible that at Wallibu the inner margin of the depressed tract may be a fault line. It has a very straight trend, and it is a curious fact that this shore was formerly known as Hot Waters. This might indicate the existence of a fissure up which hot springs were rising.

When we arrived at Martinique, we had the pleasure of meeting Professor Lacroix, the head of the French Scientific Commission, which had spent some time in making a preliminary survey of Mont Pelée and the north end of the island, and from him we obtained much valuable information regarding the sequence of events and the geological consequences of the eruptions in that quarter. It was our intention to make merely such reconnaissances as would enable us in a general way to ascertain the points of difference and of similarity between the outburst of Mont Pelée and that of the Soufrière, and to see what light the phenomena in Martinique threw on the events which had happened in St. Vincent.

Both volcanoes are of the same type, simple cones with a large vent near the summit, and without parasitic craters. They are both deeply scored with ravines, and on their south-west sides there is a broad valley—occupied at Martinique by St. Pierre city, at St. Vincent by the Wallibu. It is in these valleys that the destruction has been most pronounced. In both, the recent eruptions have been characterised by paroxysmal discharges of incandescent ashes, and a complete absence of lava streams.

In St. Vincent, however, the mass of material ejected has been much greater, and a considerably larger area of country has been devastated than in Martinique. That the loss of life was not so great can be accounted for by the absence of a populous city at the foot of the mountain. Had St. Pierre been planted at the mouth of the Wallibu valley, there can be no doubt it would have been no less completely destroyed.

On Mont Pelée, we understand that a fissure has opened on the south side of the mountain between the summit and St. Pierre, from which the blast was emitted which overwhelmed the city. But on the Soufrière the old orifices have been made use of. The eruption of Pelée began with the flow of mud lavas, but none such were seen in St. Vincent. On the other hand, the hot blast which swept down on the devoted city was essentially similar to that which we have described as having taken place at the Soufrière. Both eruptions

produced principally hot sand and dust with a small proportion of bombs and ejected blocks. The evidence of the Captain of the "Roddam," and of the survivors of the "Roraima," affords a very good idea, of what happened in St. Pierre on May 8. An avalanche of incandescent sand was launched against the city. In the north-end, which was nearest the crater, the inhabitants were instantaneously killed, the walls of the houses levelled with the ground, and the town was ablaze in a moment. In the south-end the ruin was less. Those walls of the houses which faced the crater were demolished; those which run north and south still stood, even when we were there, after the second eruption. In this quarter also all were killed, except a prisoner who was confined in an ill-ventilated cell in the prison, but we were told that for some minutes after the blast had passed people were seen rushing about in the streets, crying aloud with pain, and many threw themselves into the sea to escape the agony of their burns. It must be remembered that a terrible conflagration followed the eruption, and for 36 hours the city was a burning pile. Another eruption followed on the 18th, and cast down many of the buildings which were left. Hence it was difficult to be sure exactly what were the the effects of the volcanic blast, and what had to be ascribed to the conflagration. But we saw enough to satisfy us that the hot blast was probably no less violent here than at St. Vincent. An iron statue of the Virgin, standing on a stone pedestal on the wooded cliff overlooking the town, had been broken off and carried 40 feet away. It lay with the head pointing to the mountain, and the direction of the statue showed that the blast was travelling straight from the crater over the city. The cannon in the fort had been overthrown and had fallen away from the mountain, that is to say, in the same direction as the statue. The projecting ironwork of the verandahs of the houses was twisted and bent. The lighthouses were razed. The ships riding at anchor in the harbour, were lying side-on to the blast. Some were capsized, others had their rigging cut clean away; only the "Roddam" escaped, and she was near the south-end of the town. It was said that one man was blown clean off the "Roraima." The trees which were growing in the streets were uprooted and cast down. Many of them showed charring and sand-blast erosion on the side which faced the crater, while the lee side was still covered with the original bark.

During the minute or two which this blast lasted, so much dust fell on the "Roddam," that Captain Ford, the Harbour Master at St. Lucia, estimated that 120 tons were removed from her decks when she arrived there, and the Chief Engineer of the R.M.S. "Esk," who inspected her for Lloyd's, told us that the depth of the layer of ash was in some places 2 or 3 feet. Enough has been said to indicate the general similarity of the volcanic phenomena in Martinique

and in St. Vincent. A fuller comparison, and more particularly the investigation of the outstanding points of difference, is best deferred till the detailed results of the French Commissioners' investigations are to hand.

We were fortunate in having an opportunity of witnessing one of the more important eruptions of Mont Pelée before we left Martinique, and this enabled us to see how far the actual phenomena corresponded with the ideas we had been led to form from an inspection of the effects of the earlier outbursts. On the 9th July we were in a small sloop of 10 tons, the "Minerva," of Grenada, which we had hired to act as a convenient base for our expeditions on the mountain. The morning was spent in St. Pierre city, and among the sugar-cane plantations on the lower slopes of the mountain on the banks of the Rivière des Pères. The volcano was beautifully clear. Every ravine and furrow, every ridge and crag, on its gaunt naked surface stood out clearly in the sunlight. (See Plate 11.) Thin clouds veiled the summit, but now and then the mist would lift sufficiently to show us the jagged broken cliff which overlooks the cleft. From the triangular fissure which serves as the crater hardly a whiff of steam was seen to rise, and the great heap of hot boulders which lies on the north side of and above this fissure, could be perfectly made out. (See Plate 12.) Small land-slides took place in it occasionally, and small jets of steam rose now and again from between the stones.

A little after mid-day large steam clouds began to rise, one every 10 or 20 minutes, with a low rumble. As they rose they expanded, becoming club-shaped and consisting of many globular rolling masses, constantly increasing in number and in size as they ascended in the air. They might be compared to a bunch of grapes, large and small, or to a gigantic cauliflower. When their upward velocity diminished they floated away to leeward, and fine ash rained down in a dense mist as they drifted over the western side of the mountain. They occasioned no anxiety in our minds, as we had found that the mountain was never long without exhibiting these discharges, and they were due merely to an escape of steam carrying with it fine dust. They rose, as a rule, to heights of 5000 or 6000 feet above the sea.

That afternoon as the sun was getting lower in the heavens, and the details of ravine and spur showed a contrast of light and shadow which was absent at mid-day, we sailed along from St. Pierre to Prêcheur, intent on obtaining a series of general photographs of the hill. The steam puffs continued, and, about 6 o'clock, as we were standing back across the bay of St. Pierre, they became more numerous, though not much larger in size. We ran down to Carbet, a village $1\frac{1}{2}$ miles south of St. Pierre, where there is a supply of excellent water and good anchorage. About half-past six it was obvious that the activity of the mountain was increasing. The cauliflower clouds were no

longer distinct and separate, each following the other after an interval, but arose in such rapid succession that they were blended in a continuous emission. A thick cloud of steam streamed away before the wind so laden with dust that all the leeward side of the hill, and the sea for 6 miles from the shore, was covered with a dense pall of fine falling ash. (See Plate 13.) The sun setting behind this cloud lost all its brightness, and became a pale yellowish-green disc, easily observable with the naked eye. Darkness followed the short twilight of the tropics, but a 4 days' old moon shed sufficient light to enable us to see what was happening on the hill-side.

Just before darkness closed in, we noticed a cloud which had in it something peculiar hanging over the lip of the fissure. At first glance it resembled the globular cauliflower masses of steam. It was, however, darker in colour, and did not ascend in the air or float away, but retained its shape, and slowly got larger and larger. After observing it for a short time, we concluded that it was travelling straight down the hill towards us, expanding somewhat as it came, but not rising in the air, only rolling over the surface of the ground. It was so totally distinct in its behaviour from the ascending steam clouds that our attention was riveted on it, and we were not without apprehension as to its character. It seemed to take some time to reach the sea (several minutes at least), and as it rolled over the bay we could see that through it there played innumerable lightnings. We weighed anchor and hoisted the sails, and in a few minutes we were slipping southward along the coast with a slight easterly wind and a favourable tide. We had, however, scarcely got under way when it became clear that an eruption was impending. As the darkness deepened, a dull red reflection was seen in the trade-wind cloud which covered the mountain summit. This became brighter and brighter, and soon we saw red-hot stones projected from the crater, bowling down the mountain slopes, and giving off glowing sparks. Suddenly the whole cloud was brightly illuminated, and the sailors cried, "The mountain bursts!" In an incredibly short space of time a red-hot avalanche swept down to the sea. We could not see the summit owing to the intervening veil of cloud, but the fissure and the lower parts of the mountain were clear, and the glowing cataract poured over them right down to the shores of the bay. It was dull red, with a billowy surface, reminding one of a snow avalanche. In it there were larger stones which stood out as streaks of bright red, tumbling down and emitting showers of sparks. In a few minutes it was over. A loud angry growl had burst from the mountain when this avalanche was launched from the crater. It is difficult to say how long an interval elapsed between the time when the great glare shone on the summit and the incandescent avalanche reached the sea. Possibly it occupied a couple of minutes: it could not have been much more. Undoubtedly the velocity was terrific.

Had any buildings stood in its path they would have been utterly wiped out, and no living creature could have survived that blast.

Hardly had its red light faded when a rounded black cloud began to shape itself against the star-lit sky, exactly where the avalanche had been. The pale moonlight shining on it showed us that it was globular, with a bulging surface, covered with rounded protuberant masses, which swelled and multiplied with a terrible energy. It rushed forward over the waters, directly towards us, boiling, and changing its form every instant. In its face there sparkled innumerable lightnings, short, and many of them horizontal. Especially at its base there was a continuous scintillation. The cloud itself was black as night, dense and solid, and the flickering lightnings gave it an indescribably venomous appearance. It moved with great velocity, and as it approached it got larger and larger, but it retained its rounded form. It did not spread out laterally, neither did it rise into the air, but swept on over the sea in surging globular masses, coruscating with lightnings. When about a mile from us it was perceptibly slowing down. We then estimated that it was 2 miles broad, and about 1 mile high. It began to change its form; fresh protuberances ceased to shoot out or grew but slowly. They were less globular, and the face of the cloud more nearly resembled a black curtain draped in folds. At the same time it became paler and more grey in colour, and for a time the surface shimmered in the moonlight like a piece of silk. The particles of ash were now settling down, and the white steam, freed from entangled dust, was beginning to rise in the air.

The cloud still travelled forward, but now was mostly steam, and rose from the surface of the sea, passing over our heads in a great tongue-shaped mass, which in a few minutes was directly above us. Then stones, some as large as a chestnut, began to fall on the boat. They were followed by small pellets, which rattled on the deck like a shower of peas. In a minute or two fine grey ash, moist and clinging together in small globules, poured down upon us. After that for some time there was a rain of dry grey ashes. But the cloud had lost most of its solid matter, and as it shot forwards over our heads it left us in a stratum of clear pure air. When the fine ash began to fall there was a smell of sulphurous acid, but not very marked. There was no rain.

The volume of steam discharged must have been enormous, for the tongue-shaped cloud broadening as it passed southwards covered the whole sky except a thin rim on the extreme horizon. Dust fell on Fort de France and the whole south-end of Martinique. The display of lightning was magnificent. It threaded the cloud in every direction in irregular branching lines. At the same time there was a continuous low rumble overhead.

What happened on Mont Pelée after this discharge cannot be

definitely ascertained. For some hours afterwards there were brilliant lightnings and loud noises which we took for thunder. That night there was a heavy thunderstorm over the north-end of Martinique, and much of the lightning was atmospheric, but probably the eruption had something to do with it, and the noises may have been in part of volcanic origin.

There can be no doubt that the eruption we witnessed was a counterpart of that which destroyed St. Pierre. The mechanism of these discharges is obscure, and many interesting problems are involved. But we are convinced that the glowing avalanche consisted of hot sand and gases—principally steam; and when we passed the hill in R.M.S. "Wear" a few days later, we had, by the kindness of the captain, an excellent opportunity of making a close examination of the shore from the bridge of the steamboat. The south-west side of the hill along the course of the Rivière Sèche was covered with a thin coating of freshly fallen fine grey ashes, which appeared to be thickest in the stream valleys. The water of the rivers flowing down this part of the hill was steaming hot. This was undoubtedly the material emitted from the crater on the night of the eruption. There was no lava. We saw no explosions of combustible gases, and nothing like a sheet of flame. We were agreed that the scintillations in the cloud were ordinary lightnings which shot from one part of its mass to another, and partly also struck the sea beneath.

The most peculiar feature of these eruptions is the avalanche of incandescent sand and the great black cloud which accompanies it. The preliminary stages of the eruption, which may occupy a few days or only a few hours, consist of outbursts of steam, fine dust, and stones, and the discharge of the crater lakes as torrents of water or of mud. In them there is nothing unusual, but as soon as the throat of the crater is thoroughly cleared, and the climax of the eruption is reached, a mass of incandescent lava rises and wells over the lip of the crater in the form of an avalanche of red-hot dust. It is a lava blown to pieces by the expansion of the gases it contains. It rushes down the slopes of the hill, carrying with it a terrific blast, which mows down everything in its path. The mixture of dust and gas behaves in many ways like a fluid. The exact chemical composition of these gases remains unsettled. They apparently consist principally of steam and sulphurous acid. There are many reasons which make it unlikely that they contain much oxygen, and they do not support respiration.

After visiting Martinique we proceeded to Dominica, where Dr. Flett visited the Soufrière at the south-end of the island, and the famous Boiling Lake and Grand Soufrière. There have been few signs of increased volcanic activity here or in St. Lucia during the recent eruptions. Dr. Tempest Anderson spent some days in

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Grenada in an examination of the lagoon at St. George's, but particulars regarding these islands may be reserved till a fuller report appears.

DESCRIPTION OF THE PLATES.

Plate 11.

Mont Pelée from the West.

Taken from the sea near Prêcheur, a village north of St. Pierre. This view shows how the mountain is cut up into deep ravines by the tropical rains. Part of the summit is concealed by clouds, and the fissure from which the eruption was proceeding was apparently behind and to the right of the small central peak.

Plate 12.

Mont Pelée from the South-west.

This photograph was taken from the sea off the mouths of the Rivière Sèche and Rivière Blanche, which are about 2 miles north of St. Pierre. It shows the rugged character of the mountain, the summit of which is concealed by clouds. The slope in the foreground is the track of the avalanches which descended from the triangular (light-coloured) fissure to the right of the central peak.

Plate 13.

Mont Pelée in Eruption.

Taken from a sloop off St. Pierre on the afternoon of July 9. It shows the "cauliflower" shapes assumed by the clouds of dust and steam as they drifted westward out to sea. The lighter-coloured cloud to the east (or right) is the trade-wind cloud which so constantly covered the summit. A small light-coloured cloud-patch just below this, on the right-hand (eastern) side, indicates the fissure from which the eruption chiefly proceeded. The eruptive "avalanche" of volcanic material descended the slopes in the centre and rather to the left of the foreground. St. Pierre is to the right, outside the picture.

"On the Toxic Properties of the Saliva of Certain 'Non-poisonous' Colubrines." By A. ALCOCK, M.B., LL.D., F.R.S., Professor of Zoology, and LEONARD ROGERS, M.D., B.S. (Lond.), M.R.C.P., F.R.C.S., Officiating Professor of Pathology in the Medical College of Bengal. Received April 28,—Read June 12, 1902.

CONTENTS.

I. General Statement and Conclusions.
II. Details of Experiments.
A. With Opisthoglyphous Colubrines.
B. With Aglyphous Colubrines.
III. Tabulated Summary of Experiments.

I. GENERAL STATEMENT AND CONCLUSIONS.

Although numerous elaborate experiments, directed chiefly towards practical ends, have been made to determine the physiological effects of the parotid secretion of those Colubrine snakes whose bite is fatal to man, yet very little seems to have been done, by studying the effects of the saliva of the non-poisonous Colubrines, to assist us in forming some opinion as to how, on the theory of gradual modification by means of natural selection, the efficient lethal mechanism of the poisonous Colubrines may be supposed to have originated and become gradually perfected in all its parts.

On comparing two strong, active reptiles like the common Cobra (*Naia tripudians*) and the common Rat-snake (*Zamenis mucosus*) of this country, both of which seem to lead—and with identical success—lives that are essentially similar, two perplexing questions occur. The first question is—what is the manifest advantage to the Cobra, over the Rat-snake, of its venom? The second is, admitting that there must be some advantage—is it conceivable that it can be founded on any fundamental and unbridged difference in the nature of the saliva of the two species?

A few experiments that we have made seem to point to the conclusion that the difference is not a radical one but is only one of degree, and that the parotid secretion of some of the "harmless" Colubrines is to a certain extent poisonous when injected subcutaneously.

That the poison-gland of the venomous snakes is merely a modified

parotid gland is a generally accepted fact. That various gradations exist between an ordinary serpent's tooth and the "perforated" tooth of the venomous snakes is equally well known. If then we can show that the parotid secretion of the "harmless" Colubrines is in any degree poisonous, and if, moreover, in a series of such harmless Colubrines we can find degrees of virulence, we shall, we think, have done something towards placing the venom apparatus of the Thanatophidia at one extreme of a natural series in harmony with other facts of evolution. At one end of this series we shall have a snake like the Rat-snake (*Zamenis mucosus*) with a parotid secretion that is small in amount and only slightly toxic, and with no special means of injecting its secretion; at the other end of the series we shall have the Cobra, whose abundant parotid secretion is of lethal virulence and can be powerfully injected by a specialised fang.

We admit that certain of the links in this series have already been discovered and established, but we do not think that their full evolutional value has yet been assigned to them.

For instance, Mr. Boulenger, in his 'Catalogue of Snakes,' says of the three Colubrine sub-families *Homalopsinæ*, *Dipsadomorphinæ*, and *Elachistodontidæ*, that constitute his section of Opisthoglypha, "most, if not all, of the snakes of this division are poisonous to a slight degree, paralysing their prey before deglutition."

Again, Mr. G. S. West,* though he apparently speaks with some reserve as to the toxicity of the Opisthoglypha in general, accepts the fact that "the bite of *Dryophis* and other Opisthoglyphous snakes has been proved by several observers to be fatal to small animals."

MM. Phisalix and Bertrand,† experimenting with two European species of *Tropidonotus*, a genus of Aglyphous or "harmless" Colubrines, discovered that the secretion of the salivary glands was fatal to guinea-pigs.

Earlier still, Mr. J. J. Quelch‡ recorded of two American Colubrines —one an Opisthoglyphous or "suspicious" species (*Erythrolamprus æsculapii*), the other an Aglyphous or "harmless" species (*Xenodon severus*)—that their bite could produce enduring and painful inflammation in man.

In seeking for ourselves some independent evidence for the popular belief that "all snakes are poison," we began with the Opisthoglyphous species *Cerberus rhynchops*, *Dipsas Forstenii*, *Dryophis mycterizans*, and *Chrysopelea ornata*, all of which have parotid glands of some size and have some of the posterior maxillary teeth enlarged and grooved.

We used white mice and white rats, and we injected subcutaneously the liquid from the (perfectly fresh) crushed parotid gland, and the

* 'Proceedings of the Zoological Society for 1895,' p. 813.

† 'Comptes Rendus,' vol. 118, 1894, pp. 76–79.

‡ 'Zoologist,' 1893, pp. 30, 31.

glycerine extract—both diluted and undiluted—of the gland. But as in a series of control experiments with the secretion of the Harderian gland, with the fresh serum, and with pure glycerine, we found that the subcutaneous injection of pure glycerine is sometimes fatal to rats and mice, we have altogether eliminated the results of experiments in which glycerine extract, diluted or undiluted, was used. Our recorded experiments, therefore, refer exclusively to the effects of the liquid extracted by water or normal salt solution from the parotid gland quickly removed from the still-quivering head of the decapitated snakes.

The only specimen of *Chrysopelea ornata* that we could obtain was a very young one, from which, as we expected, we got no results. But in the case of *Cerberus rhynchops*, *Dipsas Forstenii*, and *Dryophis mycterizans* we confirmed the statements and opinions of other authors as to the venomous character—so far as small mammals are concerned—of these Opisthoglyphous *Colubridæ*.

We next tried the effect of the parotid secretion of *Tropidonotus piscator* and *Zamenis mucosus*, both of which belong to Boulenger's section of *Aglypha*, or harmless Colubrines, none of whose teeth are grooved.

As before, we used white mice and white rats, and controlled our experiments so as to eliminate the influence of blood-serum and of glycerine; and we found that, so far as these small mammals are concerned, the parotid secretion of these two "harmless" Colubrines is decidedly toxic.

Nor, even in our small series of experiments, did we fail to find some evidence of that graduated variability which any one who approaches this question from the evolution side looks for. When all allowances are made, the parotid extract of *Zamenis* appears to be considerably more potent than that of the other Aglyphous Colubrine *Tropidonotus*; and, among the Opisthoglyphous snakes, *Dipsas* seems to be decidedly more virulent than *Cerberus*, and *Cerberus* slightly more venomous than *Dryophis*. It is further worthy of notice that the poison of the two Aglyphous snakes (*Zamenis* and *Tropidonotus*) appears to act more on the nervous system, causing general convulsions, while that of the Opisthoglyphous snakes seems rather to affect the respiratory centre and to occasion respiratory convulsions.

We have made no experiments with the parotid secretion of other orders of reptiles or other classes of vertebrata, to ascertain whether or no the normal parotid saliva of animals other than snakes has any toxic properties when subcutaneously injected. Such experiments would be a natural continuation of the present inquiry. Especially would it be interesting, in view of its curious snake-like tongue and its persistently evil reputation, not only among the natives of India but also among educated Europeans, to test the saliva of the great "water-lizards" of the genus *Varanus*.

Finally we have to explain that we used the extract of the parotid gland because, even with Opisthoglyphous snakes, we were unable to devise any satisfactory way of getting parotid secretion free from the ordinary saliva of the labial glands.

II. Details of Experiments.

A. *Experiments with Opisthoglyphous Colubrines.*

i. Cerberus rhynchops.

Experiment 1.—A white mouse was injected under the skin of the back with the watery extract of both parotid glands of a full-grown snake. Ten minutes afterwards it was sluggish, 17 minutes afterwards it was lazy and sleepy, and 22 minutes afterwards its hind legs were partially paralysed. In 24 minutes the respirations were at the rate of forty in 15 seconds, and the animal could not stand. In half an hour convulsions began, and the respirations were thirty-seven in 15 seconds. At the 32nd minute the respirations had fallen to fourteen in 15 seconds, and at the 34th minute they had stopped. Thirty-six minutes after the injection the heart stopped also.

Post-mortem.—There was very marked subcutaneous extravasation of blood over the whole of the back.

No control experiments with blood-serum or with the secretion of other glands were made.

ii. Dipsas Forstenii.

Experiment 2.—A black-and-white mouse was injected under the skin of the back with the watery extract (4 minims) of one parotid gland of a full-grown snake. Twelve minutes afterwards spasmodic twitchings began. In another minute the animal turned over on its side, still twitching, and its respirations were thirty-eight in 15 seconds. At the 14th minute it could not stand, and its breathing became slower. At the 16th minute the respirations were seven in 15 seconds, and very laboured. At the 17th minute the heart was beating fast, though the breathing was occasional and convulsive. At the 21st minute after injection the breathing had stopped, and at the 23rd minute the heart.

Experiment 3.—As a control experiment, a white mouse was injected under the skin of the back with the watery extract (4 minims) of one Harderian gland of a *Dipsas Forstenii*. Thirteen minutes afterwards the animal appeared to be slightly sleepy, but no definite symptoms of poisoning were seen within the next hour, and the following day it was quite well.

iii. Dryophis mycterizans.

Experiment 4.—A small black-and-white mouse, whose respirations were about twenty-five in 15 seconds, was injected under the skin of the back with an extract of one parotid gland, in 4 minims of normal salt solution, of a full-grown snake. Two minutes afterwards the animal was restless, 3 minutes afterwards it was restless and twitching, and 5 minutes afterwards it was sleepy and nodding. At the 7th minute it was again restless, and the respirations, which were thirty-eight in 15 seconds, were deep. Twitching continued; but at the 16th minute, though the animal looked sleepy and dejected, it could walk fairly well, and appeared to be improving.

At the 26th minute a similar extract of the other parotid gland of the same snake was injected in the same way. At the 29th minute the animal was again sleepy, and its respirations were thirty-eight in 15 seconds. At the 37th minute it was very sleepy. At the 48th minute it lay quite quiet with its nose on the table, the hind legs appearing to be weak. At the 52nd minute it could not stand, and the respirations were slow and deep. At the 55th minute the respirations were only five in 15 seconds, and of a gasping character, and the animal lay helpless on its side. At the 56th minute convulsions occurred, and respiration then ceased, and just before the 57th minute after the first injection the heart stopped.

Post mortem.—There was most extensive extravasation of blood under the skin of the back and head.

Experiment 5.—A medium-sized black-and-white mouse was injected under the skin of the back with the extract of both parotid glands of a full-grown *Droyophis mycterizans* made by means of 4 minims of a normal saline solution. At the 2nd minute the animal was excited; at the 5th minute it was nodding, its respirations being then thirty-seven in 15 seconds; and at the end of a quarter of an hour it was still sleepy and breathing at the same rate, though deeper than before, with occasional twitching. At the 28th minute it was quiet, but subject to starts, its respirations being thirty-eight in 15 seconds, and of a laboured and occasionally convulsive character. At the 32nd minute the hind legs were weak, and walking was slow and difficult: the respirations were thirty-five in 15 seconds, and laboured. At the 35th minute the hind legs were paralysed and sprawling, and walking was impossible: the respirations were thirty-four in 15 seconds, and very deep. At the 37th minute the animal could just stand with its nose resting on the table: the respirations were thirty-two in 15 seconds, laboured, and occasionally convulsive. At the 39th minute the respirations had fallen to twelve in 15 seconds, and were very laboured: general convulsions then set in, the animal rolled over on its side, and breathing stopped. Forty minutes after the injection the heart stopped also.

Post mortem.—There was very marked extravasation over the back at and beyond the site of injection.

Experiment 6.—As a control experiment, a small black-and-white mouse was injected under the skin of the back with 4 minims of fresh serum of the same *Dryophis* whose glands were used in Experiment 5. The animal was closely watched for 70 minutes, but no symptoms of poisoning appeared, and it was quite well several days afterwards.

Remarks on Experiments 1, 2, 4, *and* 5.

No one who has experimented with minimal lethal doses of Cobra venom can fail to be struck with the close resemblance of the symptoms caused by it with those recorded in the above experiments with the parotid secretion of *Dryophis* and its Opisthoglyphous allies.

The gradual quickening of the respiration, the drowsiness and nodding of the head, with jerky recovery every now and then, followed by gradually increasing paralysis, and a rapid failure of the respirations after they have become laboured in character, by convulsions, and finally by stoppage of the heart some little time after the breathing has ceased, form a sequence of events that, except for a difference in intensity, are common to both, as also is the *post-mortem* picture of subcutaneous extravasation.

B. *Experiments with Aglyphous Colubrines.*

iv. Zamenis mucosus.

Experiment 7.—A black-and-white mouse was injected under the skin of the back with half the extract in distilled water of one parotid gland of a large snake. The extract, instead of being a thin opalescent fluid as in the Opisthoglyphous snakes, resembled ropy mucus. Eight minutes afterwards the animal appeared to be quite well, but in 21 minutes from the time of injection it died in violent convulsions.

Experiment 8.—A mouse was injected under the skin of the back with 1 c.c. of the watery extract of one parotid gland of a *Zamenis mucosus* nearly 7½ feet long. Five minutes afterwards it looked anxious and depressed. At 7 minutes the respirations were twenty-nine in 15 seconds. At 9 minutes the hind legs were dragging slightly. At 10 minutes the respirations were twenty-eight in 15 seconds, and the animal could walk fairly well. At 14 minutes the respirations were nineteen in 15 seconds and very laboured: the animal could still walk. At 17 minutes the respirations were ten in 15 seconds, laboured and gasping. At 18 minutes the animal was extremely restless. At 20 minutes violent convulsions and leaping occurred, which ended in the animal rolling over on its side, the

breathing having stopped, though the heart continued to beat faintly. Twenty-two minutes after the injection the heart stopped.

Experiment 9.—A white rat was injected with 1 c.c. of watery extract of one parotid gland of the same *Zamenis* used in Experiment 8. No untoward symptoms occurred.

v. Tropidonotus piscator.

Experiment 10.—A small white mouse was injected under the skin of the back with an extract of both parotid glands of two small specimens of this species, made with 4 minims of normal salt solution. Before the injection the respirations were thirty-six in 15 seconds. Seven minutes afterwards the respirations were thirty-five in 15 seconds and deeper, and at 13 minutes the respirations became laboured and the animal restless. From the 18th to the 23rd minute the respirations became more and more laboured, and increased from thirty-seven to forty in 15 seconds, the animal's restlessness and distress also increasing. At the 32nd minute the respirations, which were deep, had fallen to thirty-three in 15 seconds, the animal half reclining. A minute afterwards the respirations, which were very deep and forced, were twenty in 15 seconds. Convulsions then began, during which the animal rolled over and nearly stopped breathing. At 34 minutes breathing ceased, and just before the 36th minute from the moment of injection the heart stopped.

Post mortem.—There was marked extravasation of blood.

Experiment 11.—A small black-and-white mouse was injected under the skin of the back with the extract in distilled water of one parotid gland of a large *Tropidonotus piscator*. The fluid consisted of thick ropy mucus. Except that its respirations rose rather irregularly from thirty to forty in 15 seconds, and that it scratched itself violently at the site of the injection, the animal seemed for a long time to be all right. At 77 minutes the respirations were thirty-six in 15 seconds, and the animal seemed to be somewhat sleepy. At 92 minutes the animal was distinctly sleepy, moving sluggishly on being stirred, and dragging the hind legs. After this the animal could not be watched, but next morning (15 hours after the injection) it was found dead in its cage.

Experiment 12.—As a control experiment, a white mouse, of the same size as the one used in Experiment 10, was injected under the skin of the back with an extract in 4 minims of normal salt solution of both Harderian glands of both the specimens of *Tropidonotus* used in Experiment 10. The animal was closely watched for 40 minutes, but no ill-effects were observed. It was then put into its cage and looked at from time to time, but it was still quite well and active 6 hours after the injection, and next day its condition was quite normal.

Experiment 13.—As a control experiment another white mouse, of

III. Tabulated Summary of Experiments.

No.	Snake.	Nature of injection.	Animal.	Symptoms began.	Result.	Remarks.
1	*Cerberus rhynchops*	Watery extract of both parotids	Mouse	In 17 minutes	Died in 36 minutes	Local extravasation.
2	*Dipsas Forstenii*	Watery extract of one parotid	,,	In 12 minutes	Died in 23 minutes	,, ,,
3	,, ,, ...	Watery extract of one Harderian gland	,,	No symptoms	—	No effect.
4	*Dryophis mycterizans* ..	Normal saline solution, one parotid, repeated in 26 minutes	,,	In 5—7 minutes	Died in 56 minutes	Local extravasation.
5	,, ,, ...	Normal saline solution, both parotids	,,	In 5 minutes	Died in 40 minutes	,, ,,
6	,, ,, ...	4 minims fresh serum	,,	No symptoms	—	No effect.
7	*Zamenis mucosus*	Watery extract, half of one parotid	,,	?	Died in 21 minutes	Watery effusion.
8	,, ,, ...	Watery extract of one parotid	,,	In 9 minutes	Died in 22 minutes	Violent convulsions.
9	,, ,, ...	,, ,,	Rat	No symptoms	—	
10	*Tropidonotus piscator*	Normal saline solution, both parotids of two individuals	Mouse	In 13 minutes	Died in 35—36 minutes	Local extravasation.
11	,, ,,	Watery extract of one parotid	,,	In 77—92 minutes	Died.	
12	,, ,,	Normal saline solution, both Harderian glands	,,	No symptoms	—	No effect.
13	,, ,,	4 minims fresh serum.....	,,	,,	—	,,

the same size as those used in Experiments 10 and 12, was injected under the skin of the back with 4 minims of fresh serum of one of the specimens of *Tropidonotus* used in those experiments. No effects whatever were produced, though the animal was closely watched for a time and kept under observation until the next day.

Experiments 12 and 13 were subsequently confirmed.

Remarks on Experiments 8, 10, *and* 11.

The parotid extract of the Aglyphous snakes used was a viscid mucus, quite different from the thin opalescent fluid obtained from the Opisthoglyphous snakes. Its chemical nature would also seem to be different, the effects being much less like those produced by minimum doses of Cobra venom.

The violent general convulsions that followed the administration of *Zamenis* extract seem to point to some direct effect upon the nervous system, and are in marked contrast with the dyspnœic convulsions that characterise poisoning by the Opisthoglyphous snakes used in the first series of experiments.

"The Influence of High Pressures of Oxygen on the Circulation of the Blood." By LEONARD HILL, M.B., F.R.S., and J. J. R. MACLEOD, M.B., Mackinnon Research Scholar of the Royal Society. Received May 22,—Read June 12, 1902.

In a former communication* one of us recorded the effect of a pressure of two to three atmospheres on the circulation.

We have since carried on the observations at much higher pressures and by a different method.

A tubular steel pressure chamber was constructed. The ends of the tube were closed by thick glass discs.

A curarised frog was placed inside, and the web of one foot stretched on a wire ring just behind one of the glass discs. The pressure chamber was placed in front of an arc light, and the web illuminated so that the capillary circulation could be observed through a microscope (1-inch objective). The pressure was rapidly raised to 70 atmospheres by connecting the chamber with an oxygen cylinder.

The capillary circulation continued. No alteration could be detected during the rise of pressure. After 15–20 minutes the oxygen tap was closed and the pressure chamber rapidly decompressed. For the first half-minute there occurred no change in the circulation. Then

* 'Roy. Soc. Proc.,' 1900.

there suddenly swept down the arteries gas bubbles, which drove the blood corpuscles before them and filled the capillaries.

On recompressing the frog to 70 atmospheres the gas within the vessels passed again into solution and the corpuscles appeared in the capillaries.

We have made similar observations on a bat (obtained for us by the kindness of Mr. F. Jones).

We observed the circulation in the wing. The bat was hibernating. The circulation was therefore slow, and the heart-beat infrequent. On raising the pressure to 10 atmospheres the pulse became more frequent and the capillary circulation accelerated. At 20 atmospheres of oxygen the circulation continued unimpaired. On decompression after 10 minutes the circulation became impaired, but no gas bubbles appeared in the capillaries. The animal had not, owing to the slow circulation, been under pressure for a sufficient length of time to become saturated with gas.

One of us (L. Hill) has frequently noticed gas embolism to follow decompression of mice and birds. The gas embolism is the cause of the convulsions which follow decompression.

Conclusion.—A rapid increase of pressure to 70 atmospheres has no mechanical effect on the circulation of the blood.

This research has been carried out with the help of a grant from the Government Grant Fund of the Royal Society.

"The Influence of an Atmosphere of Oxygen on the Respiratory Exchange." By Leonard Hill, M.B., F.R.S., and John J. R. Macleod, M.B., Mackinnon Research Scholar of the Royal Society. Received May 22,—Read June 12, 1902.

Regnault and Reiset* found that the uptake of oxygen was the same in 46 per cent. and in 77 per cent oxygen as in atmospheric air.

Paul Bert† on the other hand found that the processes of oxidation were most intense in 60 per cent. oxygen, while they became lessened in a pure atmosphere of oxygen. Bert's figures for a rat placed for 24 hours in a current of air and oxygen were as follows :—

Amount of O_2 in atmosphere.	O_2 inspired.	CO_2 expired.
21·0 per cent.	12·6	7·06
48·3 ,, ...	13·72	10·32
88·2 ,, ...	11·35	6·96

* Regnault and Reiset, 'Annales de Chimie,' 20, 26 (1849). Translated in Annalen der Chemie u. Pharm.,' vol. 73, p. 92.

† Paul Bert, "La Pression Barométrique," p. 832 (Paris, 1872).

Moderate increase of the atmospheric pressure has, according to Panum and G. von Liebig,* no distinct influence on the oxygen intake or carbonic acid output of man.

Lorrain Smith also found that the oxygen tension in blood was lowered by respiration in a pure atmosphere of oxygen. The tension was estimated by the CO method.†

It will be seen from these researches that nothing conclusive can be asserted as to the influence of oxygen on the gaseous metabolism. In the following investigation, the question is studied by an entirely different method from that of previous workers, and it will be seen that we obtain very constant results.

Method employed.

The estimations were carried out on mice. These were placed in a small glass vessel, fitted with a ground glass stopper, through which passes an inlet and outlet tube. Connected with the outlet tube is a "T"-piece, through which a thermometer is passed so as to lie in the outgoing current of air. By this the temperature of the chamber was ascertained. The egress tubes were connected with Haldane and Pembrey's soda lime and sulphuric acid absorption tubes,‡ and the ingress tubes with a series of Woulfe's bottles containing soda lime and sulphuric acid, so as to remove all the carbonic acid and water from the atmosphere before entering the chamber.

A gas meter was attached to the egress tube beyond the absorption tubes. To study the effect of air an aspirating bottle was attached beyond the meter, and to study the effect of oxygen a cylinder of that gas was attached to the ingress tube.§ The O intake was determined by the difference between the loss of weight of the mouse, and the gain in weight of the CO_2 and H_2O absorption tubes.

The following tables show the results obtained :—

* G. von Liebig, 'Arch. f. dv ges. Physiol.,' vol. 10, s. 479 (1878).

† Lorrain Smith, 'Jour. Phys.,' vol. 22, 1897–98, p. 307.

‡ As described in 'Journ. Physiol.,' 1892, vol. 13, p. 419.

§ The oxygen employed was Brin's. We found this to contain from 95—97 per cent. of oxygen.

Experiment I.

Mouse weighed—at beginning ..... 19·484 grammes.
" at end ..... 17·7816 "
Length of time of experiment..... 6 hours 20 minutes.

Amosphere breathed	Air.	Air.	Oxygen.	Oxygen.*	Air.	Air.	Air.
Length of time in atmosphere at beginning of period	—	1 h. 9 m.	40 mins.	2 h. 30 m.	1 h. 50 m.	2 h. 20 m.	2 h. 40 m.
" of observation	20 min.	10 min.	30 mins.	60 min.	30 min.	10 min.	10 min.
Rate of ventilation per minute	460 c.c.	610 c.c.	220 c.c.	240 c.c.	415 c.c.	550 c.c.	1·100 c.c.
Temperature of chamber at end of period	23° C.	22° C.	21° C.	26° C.	23°·5 C.	24°·5 C.	24°·5 C.
Rectal temperature " "	38° C.	36°·5 C.	35°·6 C.	35°·8 C.	36°·2 C.	36°·2 C.	36°·4 C.
Loss of weight	0·1340 gr.	0·0555	0·0745	0·3125	0·1880	0·0590	0·0785
Amount of CO_2 expired—							
(α) During period	0·0623	0·0245	0·065	0·0926	0·0454	0·0160	0·0222
(β) Per kilogramme body weight and per minute	0·1591	0·1270	0·1134	0·0827	0·0836	0·0899	0·1240
Amount of H_2O expired—							
(α) During period	0·1235	0·0501	0·0791	0·2875	0·1827	0·0508	0·0718
(β) Per kilogramme body weight and per minute	0·3131	0·2643	0·1470	0·2525	0·3322	0·2810	0·3967
Amount of O_2 inspired† during period	0·0518	0·0191	0·0696	0·0676	0·0401	0·0078	0·0155

* The actual estimation was done in an atmosphere of air.

† The calculations were not carried further in this experiment, because there were several technical errors in the weighings of the mouse between the periods.

Experiment II.

Mouse weighed—at beginning ..... 13·8600 grammes.
" at end ..... 13·1399 "
Length of time taken in experiment ..... 6 hours 30 minutes.

Atmosphere breathed	Air.	Air.	Air.	Oxygen.	Oxygen.	Oxygen.	Oxygen.	Air.	Air.	Air.
Length of time in atmosphere at beginning of period	55 min.	2 hours	2½ hours	—	40 min.	1 h. 20 m.	2 hours	—	1 h. 20 m.	2 hours
Length of time of observation	50 min.	30 min.	30 min.	40 min.	30 min.	30 min.	30 min.	30 min.	15 min.	15 min.
Rate of ventilation per minute	300 c.c	320 c.c.	330 c.c.	450 c.c.	410 c.c.	305 c.c.	280 c.c.	380 c.c.	400 c.c.	450 c.c.
Temperature of chamber at end of period....	22	20·5	22	23	21·5	21·5	21·5	22·5	23·5	+
Rectal temperature " "	37·2	37·4	37·2	36·8	36·8	37	36	36	36	36·8
Loss of weight	0·1415	0·0888	0·1102	0·0630	0·0305	0·0195	0·0460	0·1340	0·0245	0·0250
Amount of CO_2 expired—										
(α) during period	0·1026	0·0528	0·05975	0·0780	0·0536	0·0453	0·0489	0·0433	0·0266	0·0278
(β) per kilogramme body weight and per minute	0·1478	0·1278	0·1457	0·1456	0·1333	0·1132	0·1228	0·1058	0·1291	0·1369
Amount of H_2O expired—										
(α) during period	0·1422	0·0885	0·1014	0·0810	0·0371	0·0248	0·0583	0·0311	0·0263	0·0272
(β) per kilogramme body weight and per minute	0·2020	0·2106	0·2416	0·1493	6·0800	0·0600	0·1460	0·0755	0·1291	0·1370
Amount of O_2 inspired—										
(α) during period	0·1033	0·0525	0·0509	0·096	0·0602	0·0506	0·0612	0·0596	0·0284	0·0300
(β) per kilogramme body weight and per minute	0·1443	0·1235	0·1264	0·179*	0·1500	0·1200	0·1536	0·1436	0·1367	0·1522
Respiratory quotient	0·99	0·99	1·18	0·81	0·90	0·90	0·80	0·71	0·96	0·93

* Water result too low, which accounts for O_2 result being too high.

Experiment III.

Mouse weighed—at beginning ..... 19·137 grammes.
" at end ..... 18·867 "
Length of time of experiment ..... 5 hours.

Atmosphere breathed.....	Oxygen.	Oxygen.	Oxygen.	Air.	Air.	Air.	Air.	Air.
Length of time in atmosphere at beginning of period	—	90 min.	115 min.	—	56 min.	95 min.	135 min.	175 min.
Length of time of observation	70 min.	20 min.	30 min.	45 min.	30 min.	30 min.	30 min.	30 min.
Rate of ventilation per minute.....	320 c.c.	260 c.c.	240 c.c.	250 c.c.	315 c.c.	240 c.c.	300 c.c.	—
Temperature of chamber at end of period....	20° C.	20° C.	23° C.	23° C.	22° C.	22° C.	22° ·5 C.	23° C.
Rectal temperature " "	36° C.	35°·5 C.	35° ·5 C.	35° ·5 C.	36° C.	36° C.	36 + ° C.	36 + ° C.
Loss of weight.....	0·1025 gr.	0·0180	0·0125	0·0405	0·0435	0·0655	0·0435	0·0417
Amount of CO_2 expired—								
(α) During period.....	0·1432 gr.	0 0375	0·0562	0·0816	0·0575	0·0596	0·0638	0·0641
(β) Per kilogramme body weight and per minute	0·1066	0·0978	0·0979	0·0949	0·1004	0·1044	0·1121	0·1129
Amount of H_2O expired—								
(α) During period	0·1146	0·0212	0·0320	0·0407	0·0478	0·0593	0·0547	0·0417
(β) Per kilogramme body weight and per minute	0·0835	0·05541	0·0554	0·04722	0·07886	0·1039	0·09624	0·0742
Amount of O_2 inspired—								
(α) During period.....	0·1553	0·0407	0·0757	0·0818	0·0618	0·0534	0 0750	0·0644
(β) Per kilogramme body weight and per minute	0·1180	0·1062	0·1319	0·0953	0·1083	0·0939	0·1322	0·1134
Respiratory quotient	0·93	0·92	0·75	0·99	0·92	1·11	0·85	1·0

(*Date, March* 10.)

Experiment IV.

Mouse weighed 16·75 grammes.

Estimations for periods of 5 minutes.

Atmosphere breathed	Air.	Air.	Air.	Oxygen.	Oxygen.	Oxygen.	Air.	Air.	Air.	Air.	Oxygen.	Oxygen.
Length of time in atmosphere	—	19 min.	40 min.	—	20 min.	35 min.	—	17 min.	30 min.	35 min.	20 min.	40 min.
Rate of ventilation	2·7 lit.	3 lit.	—	3·3 lit.	3·2 lit.	2·6 lit.	4·6 lit.	2·7 lit.	4 lit.	2·3 lit.	2·8 lit.	4 lit.
Temperature of chamber.	20° C.	21° C.	21° 5 C.	21°·5 C.	22° C.	22° C.	22°·5 C.	22°·8 C.	23° C.	23° C.	23° C.	23° C.
Amount of CO_2 exhaled..	0·014 gr.	0·011 gr	0·013 gr.	0·012 gr.	0·009	0·007 gr.	0·004 gr.	0 008 gr.	0·008 gr.	0·013 gr.	0·008 gr.	0·005 gr.
Amount of H_2O exhaled .	0·036 gr.	0·022 gr.	0·026 gr.	0·031 gr.	0·022	0·011 gr.	—	0 011 gr.	0·008 gr.	0·014 gr.	0·008 gr.	0·006 gr.

Experiment V.

Mouse weighed 19·26 grammes.

Estimations for periods of 5 minutes.

Atmosphere breathed	Air.	Air.	Air.	Oxygen.	Oxygen.	Oxygen.
Length of time in atmosphere	—	25 min.	1 hour.	30 min.	60 min.	90 min.
Rate of ventilation.....	2·1 lit.	1·4 lit.	1·8 lit.	3 lit.	3·6 lit.	—
Temperature of chamber.....	24°·5 C.	—	—	—	22° C.	—
Amount of CO_2 exhaled	0·019 gr.	0·017 gr.	0·018 gr.	0·014 gr.	0·012 gr.	0·007 gr.
Amount of H_2O exhaled.....	0·026 gr.	0·028 gr.	0·028 gr.	0·044 gr.	0·033 gr.	0·025 gr.

Consideration of Results.

It will be noticed that the most constant results are those of the carbonic acid. If the amount of this expired per minute and per kilo. body weight for the different periods be examined, it will be seen that there is a *very distinct diminution in the amount during respiration in a pure atmosphere of oxygen.* This diminution does not occur immediately, but is generally quite distinct in about 30 minutes.

In taking an average of the amount of this gas for any period, therefore, we have included the first period of the respiration in air which followed it.

The average of carbonic acid per minute and per kilo. body weight for a period (varying from 1 to 3 hours) *in air*, is as follows:—

No. of experiment.	Weight of mouse.*	CO_2 exhaled.
1.....	19·4 grammes	0·1331 gramme.
2.....	13·8 ,,	0·1417 ,,
3.....	19·1 ,,	0·1074 ,,

The low result in No. 3 is explained by the fact that the estimation was made after the animal had been for over 2 hours in an atmosphere of oxygen, that it had received no food during this period, and that all through the metabolism in this animal was on a lower plane than in the others.

It will further be noticed that the mouse in Experiment 2 had a higher average than that in No. 1, the difference in this case being due to the fact that the animal weighed less.†

The average for a period *in oxygen* is as follows:—

No. of experiment.	Weight of mouse.	CO_2 exhaled.
1	19·4	0·0831
2	13·8	0·1187
3	19·1	0·0993

From this it will be seen that in No. 1 there was a diminution amounting to nearly 40 per cent., in No. 2 to nearly 20 per cent., and in No. 3 to nearly 8 per cent. As it might well be argued that the diminution was not due to the effect of the oxygen, but to the fact that the animal was receiving no food, and was kept in a confined space, we must consider the *effect of an atmosphere of air following that of oxygen.* In the case of Experiments 1 and 2 a very distinct increase (viz., 20 per cent. in No. 1 and 12 per cent. in No. 2) occurred when the atmosphere was again changed to air. This increase is not marked till about 2 hours after the commencement of the period. In Experiment 3, the increase on changing from oxygen to air is about

* Weighed at beginning of experiment.

† See Schäfer's "Text-book of Physiology" (1898), vol. 1, p. 720.

8 per cent., the smaller figure in this case being accounted for probably by the more sluggish metabolism in this animal.

These results regarding the carbonic acid excretion are confirmed by those of *water excreted* and *oxygen absorbed*. For these two bodies the figures are by no means so constant as for the carbonic acid, the reason for this being no doubt that the technique for the estimation of them is much more complicated, and the chance of experimental error so much greater. The results have been further confirmed by observing the *rectal temperature* during the various periods. It will be noticed that even in an atmosphere of air a distinct fall is recorded after the animal has been in the chamber for about an hour. This fall is, however, more marked when the animal is placed in oxygen, and it again rises somewhat when the oxygen is replaced by air.

Besides the experiments here recorded, we have performed a considerable number in which the carbonic acid and water excretions alone were recorded, and in every case we have obtained the above result. Two of the most typical of these tables are given here as examples (see Experiments 4 and 5).

This research has been carried out with the help of a grant from the Government Grant Fund of the Royal Society.

"Effects of Strain on the Crystalline Structure of Lead." By J. C. W. Humfrey, B.Sc. (Vict.), 1851 Exhibition Research Scholar (University College, Liverpool), St. John's College, Cambridge. Communicated by Professor Ewing, F.R.S. Received May 28,—Read June 5, 1902.

(Abstract.)

The paper describes experiments carried out in the Engineering Laboratory at Cambridge, under Professor Ewing. The material used was a pure variety of lead which crystallised on a particularly large scale. Test pieces were obtained from it in which the part under observation (which extended right across the centre of the specimen as well as through from front to back) was a single crystal uniformly oriented throughout, and the paper deals with the behaviour of such uniformly oriented parts under the influence of strain. It was found that by suitable etching a very beautiful system of geometrical pits (which took the form of negative cubo-octahedra) could be produced on the surface of the specimen, and by means of these any change of orientation could be readily observed.

In the first experiments described the specimens were strained in

simple tension. The slip lines (the formation of which has already been described by Ewing and Rosenhain) were found to occur in such a manner that the plane of slip was parallel to an octahedral face of the etched pits. As the specimen was strained, the surface, originally plane, became slightly undulated, the undulations running diagonally across it, at an angle of about 45° to the direction of pull. When the specimen was re-etched after straining these undulations were still visible as slight changes of orientation. These undulations are apparently due to the strain not being uniform throughout the specimen. Professor Ewing has suggested to the author how a non-homogeneous strain can give rise to differences in orientation in a single isolated crystal. When slip occurs in any part, it tends to go on there rather than in other parts of the specimen. This tends to localise the strain in certain parts, while other parts of the same crystal contiguous to them have not changed their shape, or have done so in a less degree. It may thus happen that parts which were originally in parallel orientation become relatively displaced through the distortion of the material between, this acting as a strained wedge, and hence differences of orientation may arise within a single crystal.

The next experiments deal with the recrystallisation which is observed to go on in an isolated crystal of lead when it has been strained. When a single uniformly oriented crystal is in any way severely strained, it is seen upon re-etching that the orientation no longer remains uniform, but that the crystal has become broken up into numerous small patches of various new orientations. Many of the patches thus formed consist of two or more parts which bear a twin relation to one another. Change of this kind is more readily produced by compression than by tension, but it is produced by tensile strain when that is sufficiently severe.

The effect of subjecting large strained crystals to moderate temperatures (up to 100° C.) is next described, and it is shown that such cooking will produce further changes in the specimen. If the originally uniformly oriented crystal exhibits any sign of recrystallisation upon etching immediately after straining, cooking will produce a far greater amount of change, the recrystallised areas extending outwards into the parts of the crystal which before cooking had retained their original orientation. Cooking also produces a recrystallisation in specimens which show no change immediately after straining, but a certain amount of straining is always necessary before any change can be produced.

Series of photographs are given showing the gradual change which proceeds when a single crystal is cooked after being strained. From these it is seen that a process of recrystallisation continues up to a certain point, but that beyond that point, further cooking at the same temperature produces no further change. A series of photographs is

also shown illustrating the similar, though much slower, progressive growth which goes on in a strained crystal at ordinary atmospheric temperature.

The final experiments were carried out in order to decide the question, whether the recrystallisation which is apparent immediately after re-etching a severely strained crystal is a direct and instantaneous effect of the strain, or is a growth which occurs during the interval of time that has elapsed between the straining and the examination. The author has satisfied himself that the structural re-arrangement does not occur during the act of straining, like the re-arrangement (by twinning) which one can produce by straining a crystal of calcite. It occurs after the strain has taken place, during the interval of time that elapses before the specimen is etched for examination, and though it requires only a short interval of time for its development, it is to be classed with the progressive growth demonstrated by Ewing and Rosenhain in their observations on the crystals of ordinary lead after straining.

The manner in which this was demonstrated was as follows :—If the re-arrangement took place during the act of straining by successive twinning, the slip lines should, as the strain proceeded, form in new directions over the parts which had been already altered by the strain, and thus as more and more strain was given, various new systems of slip lines should appear, so grouped as to exhibit the twin character of the crystals produced. It was, however, found that the slip lines gave no indication of any such change, but ran in directions which were parallel all over the strained crystal. But when such a strained specimen was re-etched, or when a second strain was given after a short interval of time (about 5 minutes), the patches of new orientation then became visible—in the first case by the difference in orientation of the etched pits, and in the second by the appearance of numerous new systems of slip lines, parallel over certain small areas, but bearing no relation to the lines already produced by the first strain. Even when the strain was continued to the limit of fracture no re-arrangement could be observed during its application.

It is to be concluded that there is no broad distinction between the change which is visible (on etching) almost immediately after straining and that which is observed to go on after any considerable lapse of time, or when the specimen is slightly warmed.

"On the Correlation between the Barometric Height at Stations on the Eastern Side of the Atlantic." By Miss F. E. CAVE-BROWNE-CAVE, Research Student of Girton College, Cambridge, with some assistance from KARL PEARSON, F.R.S., University College, London. Received June 3,—Read June 19, 1902.

(1.) In a memoir on the correlation and variation of the barometric height at divers stations in the British Isles* by Professor Karl Pearson and Dr. Alice Lee, it is suggested (i) that interesting results might be obtained by correlating the barometer at stations on the east and west sides of the Atlantic, allowing an interval of time between the observations (see p. 459), and (ii) that with a certain distance between stations, the correlation† would be found to be negative, *i.e.*, a high barometer at the one station corresponding to a low barometer at the second (see p. 467).

(2.) In order to deal with these points, steps were taken in 1897 to collect the necessary material. Twenty years, 1879—1898 inclusive, were selected for consideration, and the early morning barometric observations for these years, copied from material provided by the kindness of the British Meteorological Office for the following East Atlantic stations:—Bødø, Florø, Skudesnaes, Valencia, Lisbon, and Funchal. These give a very fair chain of stations from the north of Norway to Madeira. On the west side of the Atlantic we obtained data for the same years for Halifax and Toronto by aid of the Director of the Canadian Meteorological Service. So far as we can judge the Canadian returns appear satisfactory and satisfactorily copied, and we have heartily to thank the Director for nominating an efficient and careful copyist. An appeal to the American Weather Bureau in Washington, and later to the Smithsonian Institute, led the latter institution to kindly offer a copy of the American data for New York, Wilmington, and Key West, free of expense to us. We regret, however, that the copy thus procured has been discovered after some work upon it to be unreliable. We still trust, however, that we may be able to proceed to work on accurate copies of the American observations.

(3.) Meanwhile a preliminary study has been made of the East

* 'Phil. Trans.,' A, vol. 190 (1897), pp. 423—469.

† The first application of the mathematical theory of correlation to meteorology occurs in the paper just cited. Samples of barometric correlation tables are there printed, and it is shown how the mean height of the barometer at Station A for a given height at Station B may be expressed in terms of standard deviations and correlation coefficients. An elementary account of correlation will be found in the 'Journal of the Roy. Statis. Soc.,' vol. 60, pp. 1—44, by G. U. Yule.

Atlantic stations, and this has impressed us with the desirability of continuing, if possible, our chain of stations right down the west coast of Africa, even to the Cape. The great mass of material to be dealt with, and the many new problems which arise in an almost entirely novel investigation of this kind, have meant of course very slow progress, and while publication of the final conclusions must be delayed for some time yet, it seems desirable to draw attention to some of the results already reached for the East Atlantic stations.

(4.) In the first place it was soon discovered that the winter and summer months (equinox to equinox) must be treated separately. It was already known that the average height varied considerably in the summer and winter months, but there are also very significant differences in the variability, and, in what we are most concerned with, *the correlation*. For example, there is hardly any correlation (0·04) between Lisbon and Valencia in the summer, but in the winter it is quite considerable (0·22). Further, the results worked out in two groups of 10 years each, show that very sensible differences in mean, variation, and correlation can exist between one decade and the next, so that at least 20 and probably more years are desirable if we are to obtain steady values for the barometric constants. In the next place while we have found a small but sensible cross Atlantic barometric correlation after a definite interval of time, we must wait for more complete American data, and for still closer investigation of the best interval for different stations before results on this point are published. The second suggestion, however, has been amply verified, and to draw attention to this is the principal object of the present preliminary notice.

As we go generally south from any station, we reach a point at which for readings on the same day there is no correlation at all. *For stations beyond this point the correlation becomes negative, reaches a negative maximum, and then begins to decrease.* Clearly it must reach a second zero. What happens after this? Does the correlation remain zero for all greater distances? To fully answer this problem we must obtain data south of Sierra Leone—in fact, we want data for St. Helena's, Ascension, and the Cape, and have taken steps to obtain them. But our confident belief is that the negative zone will be found to be followed by another zone of positive correlation, but of far less intensity. In fact, starting with any northern station and going south across the equator, we expect to reach alternate zones of positive and negative correlation, each zone marked by a successively smaller maximum. These zones are not peculiar to the earth, but rather to the station from which we start.

Thus Valencia is positively correlated with Bødø. Lisbon, however, is negatively correlated with Bødø, but positively with Valencia. We require to go as far south as Funchal to find a negative correlation

with Valencia. To get a negative correlation with Lisbon we must go as far as Sierra Leone, which has become positive again for both Bødø and Valencia. At St. Helena's we have our second negative correlation zone for both Bødø and Valencia, while we are only in the second positive zone for Lisbon. In other words, the curve of barometric correlation with distance from a station appears to give roughly the form :—

We do not find with increasing distance a diminishing correlation, as of a curve rapidly asymptoting to ox, but as it were a wave-curve of diminishing amplitude. There is not apparently an area of positive correlation surrounded by a field of zero correlation, but going south there are only *points* of zero correlation, not *regions* of zero correlation. Probably if the area of investigation can be extended we shall find *lines* not zones of zero correlation round each station, separating districts of positive and negative correlation.

Now the results which we have copied for Sierra Leone and St. Helena's are not at present complete enough to enable us to be absolutely definite with regard to them. In particular, Skudesnaes and Sierra Leone appear to give a discordant result. But our object in the present note is merely a preliminary one. We wish to call attention to a rather remarkable relation between the barometric height of stations at increasing distance, and by so doing emphasise the importance of complete returns for stations in the southern hemisphere.

(5.) The following will illustrate our point :—

Table I.

Station.	Bødø.	Skudesnaes.	Valencia.	Lisbon.	Remarks.
Bødø	1	0·6905	0·0940	−0·2815	20 years.
Skudesnaes	0·6905	1	0·4631	−0·1606	,,
Valencia ..	0·0940	0·4631	1	0·1547	,,
Lisbon	−0·2815	−0·1606	0·1547	1	,,
Funchal ..	−0·1881	−0·2305	−0·0723	0·7429	,,
Sierra Leone	+0·0079	[−0·0505 ?]	+0·0188	−0·0495	10 years.
St. Helena .	−0·0841	−0·0231	−0·0224	+0·0135	6 years (winter only).

The results for the first five stations are based on far more reliable material than those for the last two. But the latter suffice to suggest

such a conclusion as that the barometer in the north of Norway may be as closely related to a station—St. Helena in the southern hemisphere—as to Valencia in its own; the correlation for the *winter* months of Valencia and Bødø is 0·0590, while its correlation with St. Helena for the winter months is – 0·0841. Valencia, far nearer to St. Helena, is less highly correlated with it than Bødø is, while Bødø itself is nearer related to St. Helena than to Sierra Leone. The relationship here is not, be it remembered, a relationship of average heights, but of the daily fluctuations of the barometer at the two stations—*i.e.*, it would be safer to predict the barometric height in the winter at Bødø on any day from the height on the same day at St. Helena, than to predict it from the height at Valencia. Results of this kind seem of sufficient interest to deserve notice, even before the whole material has been collected and reduced.

What we are certain about is, that a zone of positive correlation is followed by a zone of negative correlation. What we are less sure about is, that this negative zone is again followed by a positive zone of much less intensity, but our rather meagre results certainly suggest it.

(6.) The accompanying tables give the more complete results for the five more northerly stations.

Table II.—Height and Variation of Barometer at East Atlantic Stations.

Means in mm.

Period.	Bødø.	Skudesnaes.	Valencia.	Lisbon.	Funchal.
1879–88. Summer.....	757·006	758·324	760·285	763·328	764·558
„ Winter.....	753·315	757·866	760·013	765·516	765·030
„ Whole year..	755·194	758·099	760·151	764·402	764·790
1889–1898. Summer...	758·174	759·008	761·160	763·199	764·721
„ Winter....	754·171	757·927	760·278	764·895	764·929
„ Whole year	756·210	758·478	760·727	764·031	764·823
1879–1898. Summer...	757·590	758·666	760·722	763·264	764·639
„ Winter ...	753·743	757·896	760·145	765·206	764·979
„ Whole year	755·702	758·288	760·439	764·217	764·806

For brevity, the probable errors are not at present given, but the differences between the two decades are as a rule sensibly larger than the probable errors. Several interesting results flow from this table. Thus, north of Valencia, the summer barometer is higher than the winter; south of Valencia, the winter higher than the summer. In al cases the winter height is more variable than the summer, but the

Standard Deviations in mm.

Period.	Bødø.	Skudesnaes.	Valencia.	Lisbon.	Funchal.
1879–88. Summer.....	7·391	7·573	7·746	3·812	3·064
,, Winter.....	11·056	11·299	10·508	6·079	4·781
,, Whole year..	9·551	9·588	9·207	5·171	4·007
1889–1898. Summer...	7·837	7·501	7·227	3·393	2·964
,, Winter....	10·999	11·146	10·228	6·253	5·053
,, Whole year	9·729	9·482	8·839	5·076	4·125
1879–1898. Summer...	7·640	7·545	7·504	3·609	3·015
,, Winter....	11·036	11·223	10·370	6·175	4·919
,, Whole year	9·653	9·537	9·030	5·127	4·066

winter variability tends to approach the summer variability the further south we go*; the absolute variability decreasing, however, as we go south.

Table III.—Correlation of the Barometric Heights at the East Atlantic Stations.

(See attached sheet.)

Now it will be seen that from these stations, for which our results are very reliable, that

(i.) The correlation between certain stations is sensibly different for summer and winter.

(ii.) The correlation varies somewhat for the same pair of stations from decade to decade.

(iii.) In every case beyond a certain distance from a station the correlation becomes negative, reaches a negative maximum, and then begins to decline.

Now we refrain from either plotting curves for these changes in correlation or from drawing inferences as to the nature of the instantaneous distribution of barometric height along a meridian until we have been able to deal with more complete data from equatorial stations, and from stations south of the equator. But we believe this alternation of instantaneous high and low barometer† along a meridian, and the discovery of uncorrelated stations, are of sufficient physical interest to deserve immediate notice. We propose to call a station, uncorrelated with a given station, a station *nodal* to it. Thus to every station will exist a *first nodal line* of stations, the barometric height at which is uncorrelated with that at the given station. Within this

* There is an apparent exception to this in Bødø and Skudesnaes.

† "High" and "low" barometer at any station mean here above and below the mean for that station.

first nodal line there will be positive correlation, beyond it negative correlation, until we reach the *second nodal line*; beyond this the correlation may become again positive, and so on. All nodal lines appear to be relative to a given station, and are not to be considered as curves peculiar to the earth's surface.

One important result of this system of places nodal to a given station is, that it probably depends on the interval of time between the observations at the two stations. Accordingly, in endeavouring to predict the height of the barometer on the east side of the Atlantic from an antecedent series of heights on the west side, we might easily fail to get satisfactory results if the stations on the west side were approximately nodal to one or more stations on the east side. In this case we might do better to take our stations *further to the west*, or nearer the maxima of the next internodal zone.

We hope shortly to complete our calculations to the Cape, and then to finish the work already begun on the American stations. Meanwhile, we think that the correlation of a series of stations following roughly a parallel of latitude across Europe and Russian Asia would throw a flood of light on whether a chain of roughly north and south stations differs wholly in character from a chain of east and west stations. The magnitude of the computations, however, almost precludes the idea that any individual worker or workers can hope to complete such a task within a reasonable period.

Place.	Period.	Lisbon.		Funchal.		
		Winter.	Whole year.	Summer.	Winter.	Whole year.
Bødø	1879—88	0·2919	−0·2697	−0·0349	−0·2291	−0·1772
	1889—98	0·3299	−0·2906	−0·0346	−0·2707	−0·1993
	1879—98	0·3124	−0·2815	−0·0326	−0·2505	−0·1881
Skudesnaes	1879—88	0·1430	−0·1513	−0·1461	−0·2787	−0·2387
	1889—98	0·1681	−0·1691	−0·1527	−0·2525	−0·2227
	1879—98	0·1556	−0·1606	−0·1479	−0·2642	−0·2305
Valencia ..	1879—88	0·1862	+0·1301	−0·0926	−0·1315	−0·1191
	1889—98	0·2514	+0·1837	−0·0630	−0·0077	−0·0255
	1879—98	0·2178	+0·1547	−0·0768	−0·0688	−0·0723
Lisbon ..	1879—88	—	—	+0·7792	+0·7194	+0·7315
	1889—98	—	—	+0·7213	+0·7768	+0·7559
	1879—98	—	—	+0·7509	+0·7488	+0·7429
Funchal ..	1879—88	0·7194	+0·7315	—	—	—
	1889—98	0·7768	+0·7559	—	—	—
	1879—98	0·7488	+0·7429	—	—	—

Table III.—East Atlantic Stations. Barometric Correlation.

Place.	Period.	Bødø.			Skudesnaes.			Valencia.			Lisbon.			Funchal.		
		Summer.	Winter.	Whole year.	Summer.	Winter.	Whole year.	Summer.	Winter.	Whole year.	Summer.	Winter.	Whole year.	Summer.	Winter.	Whole year.
Bodo	1879—88	—	—	—	+0·6869	+0·6930	+0·6824	+0·1950	+0·0688	+0·1120	−0·1165	−0·2919	−0·2697	−0·0349	−0·2291	−0·1772
	1889—98	—	—	—	+0·7197	+0·6954	+0·6988	+0·0938	+0·0479	+0·0725	−0·1125	−0·3299	−0·2906	−0·0346	−0·2707	−0·1993
	1879—98	—	—	—	+0·7040	+0·6937	+0·6905	+0·1482	+0·0590	+0·0940	−0·1151	−0·3124	−0·2815	−0·0326	−0·2505	−0·1881
Skudesnaes	1879—88	+0·6869	+0·6930	+0·6824	—	—	—	+0·4771	+0·4944	+0·4882	−0·1652	−0·1430	−0·1513	−0·1461	−0·2787	−0·2387
	1889—98	+0·7197	+0·6954	+0·6988	—	—	—	+0·3854	+0·4588	+0·4361	−0·1489	−0·1681	−0·1691	−0·1527	−0·2525	−0·2227
	1879—98	+0·7040	+0·6937	+0·6905	—	—	—	+0·4344	+0·4770	+0·4631	−0·1579	−0·1556	−0·1606	−0·1479	−0·2642	−0·2305
Valencia ..	1879—88	+0·1950	+0·0688	+0·1120	+0·4771	+0·4944	+0·4882	—	—	—	+0·0333	+0·1862	+0·1301	−0·0926	−0·1315	−0·1191
	1889—98	+0·0938	+0·0479	+0·0725	+0·3854	+0·4588	+0·4361	—	—	—	+0·0580	+0·2514	+0·1837	−0·0630	−0·0077	−0·0255
	1879—98	+0·1482	+0·0590	+0·0940	+0·4344	+0·4770	+0·4631	—	—	—	+0·0434	+0·2178	+0·1547	−0·0768	−0·0688	−0·0723
Lisbon ..	1879—88	−0·1165	−0·2919	−0·2697	−0·1652	−0·1430	−0·1513	+0·0333	+0·1862	+0·1301	—	—	—	+0·7792	+0·7194	+0·7315
	1889—98	−0·1125	−0·3299	−0·2906	−0·1489	−0·1681	−0·1691	+0·0580	+0·2514	+0·1837	—	—	—	+0·7213	+0·7768	+0·7559
	1879—98	−0·1151	−0·3124	−0·2815	−0·1579	−0·1556	−0·1606	+0·0434	+0·2178	+0·1547	—	—	—	+0·7509	+0·7488	+0·7429
Funchal ..	1879—88	−0·0349	−0·2291	−0·1772	−0·1461	−0·2787	−0·2387	−0·0926	−0·1315	−0·1191	+0·7792	+0·7194	+0·7315	—	—	—
	1889—98	−0·0346	−0·2707	−0·1993	−0·1527	−0·2525	−0·2227	−0·0630	−0·0077	−0·0255	+0·7213	+0·7768	+0·7559	—	—	—
	1879—98	−0·0326	−0·2505	−0·1881	−0·1479	−0·2642	−0·2305	−0·0768	−0·0688	−0·0723	+0·7509	+0·7488	+0·7429	—	—	—

Number of Observations used.

Period.	Summer.	Winter.	Whole year.
1879—1888	1860	1793	3653
1889—1898	1860	1792	3652
1879—1898	3720	3585	7305

"On the Movements of the Flame in the Explosion of Gases." By HAROLD B. DIXON, M.A., F.R.S. Received and Read June 5, 1902.

(Abstract.)

PART I.—*Historical Introduction.*

Bunsen, in 1867, made the first careful measurement of the rate at which an explosion is propagated in gases, and he also made the first systematic researches on the pressure and temperature produced by the explosion of gases in closed vessels. His results led him to the remarkable conclusion that there was a discontinuous combustion in explosions. When electrolytic gas, or when carbonic oxide with half its volume of oxygen is fired, only one-third of the mixture is burnt, according to Bunsen, raising the temperature of the whole to about 3000° C. No further chemical action then occurs until the gaseous mixture falls by cooling below 2500°. Then a further combustion begins, and so on, *per saltum.* These deductions were criticised by Berthelot, who pointed out that they assumed the constancy of the specific heats of steam and of carbonic acid at high temperatures.

Bunsen also stated that the rapidity with which the flame of the explosion spreads is synchronous with the attainment of complete combustion and of the maximum temperature.

In 1881 Berthelot and Le Chatelier independently discovered the great velocity with which the flame travels in gaseous explosions. Berthelot showed that this velocity was a constant for each gaseous mixture, and compared the rate of the "detonation-wave" (*l'onde explosive*) with the mean velocity of the molecules produced by the combustion before they had lost any heat. In the Bakerian Lecture for 1893, the author showed that Berthelot's theory did not account for many observed rates of explosion, and put forward the view that the explosion-wave travelled with the velocity of sound in the burning gases. Using the rates determined by the author, D. L. Chapman has argued that, if the explosion-wave is of a permanent type, an equation can be deduced from Riemann's formula by which the rates of explosion can be calculated if the specific heats are known, and *vice versâ.* The rate of the detonation-wave may therefore be utilised, according to Chapman, to determine the specific heats of gases at very high temperatures.

In 1883 Mallard and Le Chatelier published their researches on the combustion of gaseous mixtures. Using a delicate indicator, they found that rapidly exploding gases gave very high pressures for very small periods of time; these high but fugitive pressures they attribute

to the compression-wave which is propagated as the inflammation spreads from layer to layer. To obtain the mean pressure of the ignited mass of gas they had recourse to a less sensitive Bourdon gauge, and from the pressure-curves so registered they calculated the maximum pressures and temperatures of the explosion. Their results may be summarised in the statement that the maximum temperature of the explosion of moist electrolytic gas is 3350° C., and the mean specific heat of steam between that temperature and 0° is 16·6 (at constant volume), dissociation being very slight, if any, between these temperatures; on the other hand, the mean specific heat of CO_2 rises to 13·6 at 2000°, and above this dissociation begins. The simple diatomic gases (O_2, N_2, CO, &c.) show a rise of specific heat, though far less marked.

Berthelot and Vieille (1885) also determined the maximum pressures produced in the explosion of gases, and calculated the maximum temperatures. Their results were similar to those obtained by Bunsen, but they attribute the defect of pressure observed not to the inability of the gases to combine at the temperature reached, but to the great increase of the specific heats of the products of combustion.

On the other hand, Dugald Clerk contended (1886) that in an explosion the combustion is never completed instantaneously, and since the burnt gases are cooling while the unburnt are still combining, the observed pressures and temperatures fall short of those calculated for instantaneous combustion.

Mallard and Le Chatelier were the first to record the movements of the flame in explosions by photography on a moving film (1883). Failing to obtain images of the flame with mixtures such as $2CO + O_2$, they employed CS_2 with oxygen and with nitric oxide.

When the gases were ignited by a flame at the open end of a long tube, the flame was propagated along the tube for some distance with a uniform slow velocity. In the case of mixtures of carbon disulphide with nitric oxide, this period of uniform movement is succeeded by oscillations of the flame, which sometimes become of larger and larger amplitude and then die down, and sometimes give rise to the "detonation-wave." When carbon disulphide is mixed with oxygen, the preliminary period of uniform movement is shorter, and is succeeded immediately by the detonation.

Mallard and Le Chatelier draw attention to the fact that in these explosions—starting at the open end of a tube—the *development of the detonation-wave is not progressive, but always instantaneous.* When the mixture was fired near the closed end of a tube, the movement of the flame was uniformly accelerated until the detonation is set up. Their apparatus did not move fast enough to analyse the more rapid movement of the flame.

In 1888 von Oettingen and von Gernet analysed the flame by

means of a rotating mirror. Their photographs reveal, they state, the phenomena immediately succeeding the chemical combustion.

For although the flame of electrolytic gas appeared intensely bright, its spectrum only gave the sodium and calcium lines, and the most sensitive photographic plates showed "hardly a trace of the process." Failing to photograph the flame itself, the authors added finely divided salts to the tube, and found that the most brilliant pictures were given by cuprous chloride.

Their pictures show the passage of waves sharply reflected backwards and forwards from the ends of the tube, and gradually diminishing in intensity and velocity. These visible waves, according to von Oettingen and von Gernet, are not a picture of the process of combustion itself, but are compression-waves moving through the products of combustion after the explosion is completed. *The explosion itself*, they say, *is quite invisible.* Their photographs also show waves running nearly parallel with the primary waves. These secondary waves are particularly referred to as supplying conclusive evidence that *successive partial explosions* have taken place (starting from the electrodes) exactly as Bunsen imagined.

In 1884 Liveing and Dewar showed that the flame of an explosion in a glass tube exhibited the spectral lines of sodium and of calcium, and of iron when fired in an iron tube and examined end-on. When metallic salts were introduced in the form of powder, the corresponding lines were visible in the spectroscope. They made the interesting observation that the red lithium line was *reversed* when the explosion was made to travel towards the spectroscope, and they interpreted this to mean that the front of the advancing wave is cooler than the following part.

I have examined the spectra of many explosion-flames. The light produced by the explosions of electrolytic gas is mainly due to particles knocked from the glass. In the faint continuous spectrum shown by the flame the calcium lines stand out prominently. When the explosion travels first through a metal tube joined to a glass one in which the flame is photographed, the light is more intense near the junction. One can see the stream of luminous matter carried out of the metal tube.

The cyanogen explosions, however, give a continuous spectrum crossed by metallic lines and by the characteristic "cyanogen lines."

The luminous particles, whatever their nature, follow very closely the movements of the gas in which they float. When the spectra of the explosion-flames were photographed "end-on," I have never observed any reversal of a line in the advancing flame.

PART II.—*Photographic Analysis of Detonation-waves and their Reflections.*

[*In conjunction with* E. H. STRANGE, B.Sc., *and* E. GRAHAM, B.Sc.]

By throwing the image of the explosion tube on to a photographic film (Eastman's) fixed to a rapidly revolving wheel, we found that the flame could be sharply photographed, and its movements analysed, without the addition of any metallic salts to the tube.

The first point noticed in the photographs were (1) the sharpness with which the luminosity is set up; and (2) the uniformity of the detonation-wave. There is no evidence of any gradual heating up of the gases, but, on the contrary, the temperature appears to spring to its maximum with abrupt suddenness. The gas ignited by the detonation (including particles knocked off the tubes) remains luminous for some time after the wave has passed.

Many of the photographs show very distinctly the movements of the gas *en masse*, as it follows up the detonation-wave, comes to rest, and swings back again.

When the detonation-wave hits the closed end of the tube it is reflected back in a distinctly marked luminous wave, remarkable for its great luminosity. As this reflected wave starts back from the closed end it has at first to meet the gas moving bodily forward in the wake of the detonation-wave. As it continues backwards the gas it meets has less forward motion, then becomes stationary, and finally travels back in the same direction as the reflected wave. It follows, therefore, that the velocity of the reflected wave is at first retarded and afterwards increased by the motion of the medium.

The reflected wave produced by the collision of a detonation-wave with the closed end of the tube is mainly an intense compression-wave. The velocity of the reflection-wave may be readily compared with that of the detonation-wave. In the following table the average velocities observed in several gaseous mixtures are given, the velocity of the reflected wave being taken as nearly as possible at the point where the movement of the gas itself was nil.

Although the formula for the velocity of sound in gases is strictly valid for small displacements only, nevertheless it appeared of interest to calculate from the observed velocities of these reflection-waves what temperature they indicated in the gas, on the assumption that they were propagated as sound-waves. Of course to calculate the temperature from the velocity of sound it is necessary to know the ratio of the specific heats γ, and since in the case of carbonic acid and steam this ratio is very doubtful, a corresponding uncertainty must exist in the temperature calculated. But in the case of cyanogen burning to carbonic oxide, the products of combustion, carbonic oxide, and nitrogen are similar to air, and their specific heats either do not alter, or do not alter greatly, with rise of temperature. The velocity

Table I.—Velocity of Reflection-waves in Gaseous Explosions.

Mixture of gases.	Velocity of explosion-wave in metres per second.	Velocity of reflection-wave in metres per second.	Ratio of velocities.
$2H_2 + O_2$	2820	1538	1·83
$H_2 + N_2O$	2305	1383	1·67
$2CO + O_2$	1676	1078	1·56
$C_2N_2 + O_2$	2728	1230	2·22
$C_2N_2 + 2O_2$	2321	1129	2·06
$2C_2H_2 + 5O_2$	2391	1133	2·11

of sound in such a gas would therefore give an approximation to the temperature.

Now the velocity of the reflection-wave in cyanogen exploded with its own volume of oxygen is 1230 metres per second. Assuming γ to be unaltered by rise of temperature, and the velocity of sound in air at 0° C. to be 333 metres per second, the temperature of the gas where the reflection-wave was measured is given by the formula—

$$T = \left\{\left(\frac{V\sqrt{\frac{d_1}{d}}}{333}\right)^2 - 1\right\}273 = 3330^\circ \text{ C.},$$

where V is the velocity of sound, and d_1 and d the densities of the gas and air respectively under the same conditions. If, on the other hand, we assume (with Le Chatelier) that the specific heat at constant volume of diatomic gases rises with the temperature and becomes 7 at the temperature of this experiment, then the ratio γ_1 falls to 1·29, and the formula becomes:—

$$T = \left\{\left(\frac{V\sqrt{\frac{d_1}{d}\cdot\frac{\gamma}{\gamma_1}}}{333}\right)^2 - 1\right\}273 = 3672^\circ \text{ C.}$$

In the case of cyanogen exploded with twice its volume of oxygen, the first reaction probably consists in the burning of the cyanogen to carbonic oxide, which combines more slowly to form carbonic acid. How far this second reaction is completed when the reflection-wave is measured, it is impossible to decide. On the assumption that the specific heat of nitrogen is constant, and that CO_2 is 7·2, the velocity of the wave in the completely burnt mixture indicates a temperature of 4200° C.; on the assumption that the specific heats of CO_2 and N_2 are 20 and 7, the temperature indicated is 4780° C. On the other hand, if no carbonic acid had yet been formed, the temperature indicated for the mixture of diatomic gases ($2CO$, O_2, N_2) is 2880° C. ($C_V = 4{\cdot}8$).

In a similar manner the temperatures corresponding to the velocity of the reflection-waves have been calculated for the other mixtures, (1) assuming the ratio of the specific heats for a diatomic gas to be 1·41, and for a triatomic gas 1·28, and (2) assuming the ratio of the specific heats for a diatomic gas to be 1·29, and for a triatomic gas 1·11:—

Table II.—Temperatures of Exploded Gases calculated from the Velocities of the Reflection-waves.

Mixture.	I. γ for diatomic gas = 1·41. γ for triatomic gas = 1·28.	II. γ for diatomic gas = 1·29. γ for triatomic gas = 1·11.
$2H_2 + O_2$	3720° C.	4830° C.
$H_2 + N_2O$	3660	4130
$2CO + O_2$	4530	5250
$C_2N_2 + O_2$	3330	3670
$C_2N_2 + 2O_2$	4200	4780
$2C_2H_2 + 5O_2$	3980	4630

A glance at this table reveals the fact that, whether the specific heats vary or not, but on the assumption that combustion is complete in each case, the explosion of cyanogen to carbonic oxide, which, according to all observers, gives the brightest flash, and the highest pressure, also gives (apparently) the coolest combustion products a short time after the explosion-wave has gone by. The natural inference to be drawn from these figures is that in those mixtures where steam or carbonic acid, or both, are produced, the combustion is not complete at the moment the reflection-wave is measured.

Part III.—*On the Velocity of a Sound-wave in the Flame of Exploded Gases.*

[*In conjunction with* R. H. Jones, B.Sc., *and* J. Bower, B.Sc.]

The interest attaching to the determination (even approximately) of the temperatures produced in the explosion of gases led us to attempt the measurement of the rate of a true sound-wave (of small displacement) in the gases produced by the detonation-wave.

In our first experiments the glass explosion-tube was fitted to a steel piece containing a tap of large bore, and a small bye-tap, and connected by a pipe to a steel bomb, in which a small charge of fulminate could be fired. The bomb and connecting pipe were filled with air, while the tube was filled with a mixture of cyanogen with two volumes of oxygen.

The lengths of the tubes were so adjusted that the sound-wave, started in the bomb by the detonation of the fulminate, should be

propagated through the air and cyanogen mixture, so as to meet the detonation-wave coming in the contrary direction before the latter reached the end of the tube. The detonation-wave was then photographed as it met the sound-wave. The photographs clearly show several sound-waves passing through the incandescent gases.

The rates of these sound-waves have been measured and the corresponding temperatures calculated. These values are given in Table III on the assumption that the combustion was complete.

Table III.

Number.	Velocity of sound-waves in explosion of $C_2N_2 + 2O_2$.	Calculated temperature. γ for diatomic gas, 1·41. γ for triatomic gas, 1·28.
1st	1116 metres per sec.	4100°
2nd	1014 ,,	3330
3rd	893 ,,	2530

It will be seen that the temperature calculated for the first sound-wave (4100°) is in close accordance with that calculated from the reflection-wave in the same mixture (4200°) given in Table II.

The experiment was next varied by the introduction of a thin iron membrane between the air and the explosive mixture. The shock transmitted through the air from the fulminate struck the flexible plate, and so propagated a wave of small displacement through the explosive mixture. This wave had very little effect on the movements of the gas in the wake of the detonation-wave, but its passage through the luminous gas was plainly marked. The gases were ignited as before, the lengths of the tubes being so adjusted that the first sound-wave met the detonation-wave about 1 metre from the membrane.

With the mixture $C_2N_2 + O_2$ three photographs were obtained in which the course of the sound-waves were fairly marked. The mean of several independent measurements made on each photograph gave as the velocity of the sound-wave *in the stationary gas* as 1250 metres per second. This velocity corresponds to a temperature of 3460° ($\gamma = 1{\cdot}41$), a number in very fair agreement with that calculated from the reflection-waves, viz., 3330° (Table II). This agreement indicates that the reflection-waves really travel with a velocity approximately equal to that of sound.

PART IV.—*On the Collision of Two Detonation-waves, and the Effect of Junctions in the Tubes.*

[*In conjunction with* R. H. JONES *and* J. BOWER.]

To study the effect produced in the collision of two waves of detonation, the explosion tube (of lead pipe) was bifurcated into two arms of equal length which were bent round and held the two ends of a strong glass tube, in the centre of which the two waves met. The joint between the lead and glass was made tight with india-rubber.

The photographs obtained with this apparatus were puzzling. Some of the "rebound waves" after the collision were much brighter and travelled (backwards) much faster than the detonation-waves themselves. Moreover, many photographs showed that the flame of the explosion had been affected by some impulses causing a sudden increase in its brightness and velocity, and producing a backward wave (analogous to a reflected wave).

The explanation of these appearances that first occurred to us was that the flame was preceded by invisible sound-waves, travelling more quickly than the flame in its initial phases; that these sound-waves became visible as soon as they met the flame moving towards them in the opposite direction (as in our previous experiments on sound-waves), and that, on the other hand, the visible flame meeting the sound-wave was affected by the sudden increase of pressure, and continued its journey with greater speed and luminosity. This explanation was at once destroyed when we found similar impulses in a flame which was sent through the apparatus in one direction only.

It next occurred to us that these impulses might be due to the explosion *catching up its own sound-waves.* If sound-waves are propagated through the gas from the point of ignition, the flame might lag behind the sound-waves at first and catch them after a run more or less prolonged. The sound-waves when overtaken might cause reflected sound-waves (made visible in the luminous gases), and the explosion itself might become more intense owing to the collision. Many experiments were undertaken to verify or disprove this hypothesis; but finally it was found that the explosion was affected as it passed through the junctions between the lead and the glass, and the "impulses" recorded in our photographs were due to the detonation-wave, damped down at the junction, being regenerated by fits and starts.

The flame is not retarded by turning round a corner, even when that is a sharp angle, nor is it damped down when the connecting junction is made of stout flexible rubber. After many trials we found that the only thing which mattered was the rigidity with which the glass and metal were connected together. Any packing (such as

rubber) which gave to a shock caused a retardation; when the glass was firmly cemented to the metal no retardation occurred. The anomalies met with in our collision experiments were thus accounted for, and the examination of collisions between true detonation-waves was proceeded with.

When two detonation-waves come into collision the tube remains brightly luminous at the point of contact for some time, and two reflected waves are sent backwards with velocities which increase at first, owing to the movement of the gas through which they are propagated.

A comparison of all the photographs shows that the gases are more luminous after a collision than when the explosion-wave strikes a flat surface of metal fastened at the end of the tube. The reflected waves in the two cases are similar in character, but the reflection generated by collision with another detonation-wave seems always to travel slightly faster. If we were dealing only with waves produced mechanically, the reflected waves would be exact copies of the incident waves with velocities reversed—in both cases. But in the detonation-wave we have chemical as well as mechanical action, while the reflected wave is mainly mechanical. We should expect therefore the reflected waves to travel more slowly than the incident waves, but we should also expect the reflected waves to travel with the same velocity whether they were produced by collision with a rigid diaphragm or with a similar and equal wave travelling in the opposite direction—*unless there was some chemical difference involved in the two kinds of collision.*

Our photographs have shown that the wave of detonation has certain characteristics by which it may readily be recognised :—

(1.) It starts suddenly, throwing back a strongly luminous wave through the burning gases, and leaving a dark space where it started.

(2.) It travels with constant velocity unless it traverses a junction not rigidly attached; after being damped down by such an obstacle it recoups itself and again starts with abruptness.

(3.) On collision with a similar detonation-wave moving in the opposite direction, or with a rigid diaphragm, it sends back a reflected wave not so rapid as itself, and as a rule not so luminous.

In the case of the more luminous explosions, *e.g.*, those of cyanogen, acetylene, and carbon disulphide mixtures, the reflected waves were less luminous than the detonation-wave; but in the case of the less luminous explosions, *e.g.*, those of hydrogen and carbonic oxide, which depend largely for their light on the particles detached from the tubes, the waves reflected from a collision were sometimes more luminous than the detonation-waves themselves.

Part V.—*On the Initiation of the Detonation-wave and on the Wave of Retonation.*

[*In conjunction with* R. H. Jones *and* J. Bower.]

Our photographs show abrupt changes in the acceleration of the explosion before the final spring which marks the detonation-wave. These sudden changes are accompanied by a luminous wave thrown back through the ignited gases.

The strongly luminous wave thrown back from the point where the detonation is started we call the "Retonation-wave." A study of a number of photographs leads to the conclusion that the retonation is faster and more luminous when no other bright waves have been thrown back by the advancing flame before the point of detonation is reached.

The collision of two flames, in which detonation had not yet been determined, gave rise to reflected waves more rapid and more luminous than the incident waves. Now these reflected waves could not owe their increased velocity to the mechanical impact, which could only result in the reflected waves being copies of the incident waves. It is evident then that chemical action must occur to assist these reflected waves, and, therefore, the combustion is obviously not complete when these waves return. From this it would appear probable that the period before the detonation is distinguished not only by a slower propagation of the flame, *i.e.*, of ignition, but also by a slower process of combustion.

At the point of detonation the rapid rise of pressure produces not only the forward wave—that of detonation—but also a backward wave of compression into the gases still slowly burning behind it. This compression-wave must raise the temperature of the ignited gases and quicken the residual burning; its propagation would thus be analogous to that of the detonation-wave, but modified by the extent to which the slow combustion had proceeded.

The retonation-wave attains its greatest rapidity and brightness when it is developed at the closed end of a tube, *i.e.*, when the gas is fired at such a distance from the closed end that the explosion, gradually increasing in intensity, just reaches the detonation point as it arrives at the stopper. Under such conditions the reflected wave is superposed on the wave of retonation, and the result is a wave which cannot be distinguished from a detonation-wave.

As regards the dark space formed at the point where the detonation- and retonation-waves originate, it is no doubt a space of cooler gas. It persists for some time, and its damping effect on the passage of collision-waves can be observed in several of the photographs.

PART VI.—*On the Initial Phases of the Explosion.*

[*In conjunction with* R. H. JONES *and* J. BOWER.]

As several of our photographs had shown that the flame in the period of acceleration was overtaken by a more rapidly moving wave, we photographed the *beginning* of an explosion of cyanogen with oxygen, and found that the bright flame which overtook the primary flame came *from the end of the tube near the firing wires.* The "kick-off" which the explosion gets from the closed end, apparently enables the reflected wave to overtake the original wave of combustion. When the gases are fired by means of wires *just* penetrating the stopper closing the end of the tube, the explosion proceeds more slowly, and without disturbance by any sudden changes of acceleration until at last the point of detonation is reached. The photographs of these explosions are in marked contrast to those taken when the firing wires were 3 inches from the end of the tube. In the less rapid explosions it is seen that the flame does not travel direct to the near end of the tube, but while still a short distance from it recedes and again approaches with an oscillatory motion, which is repeated before the flame finally reaches the end of the tube. From the point where the flame is first checked a luminous wave is seen running back and overtaking the main flame, which at this point acquires greater brightness and velocity. From the point of collision where the faster overtakes the slower flame, a reflection is thrown back to the near end of the tube where it is again reflected.

Now when an explosive mixture is fired by a spark, the suddenly ignited gases must expand and transmit a compression-wave in both directions. This travels with the velocity of sound in the unburnt gas, and will be reflected from the end of the tube. The propagation of the flame from the firing point is, in most gaseous mixtures, less rapid than the velocity of sound in the unburnt gas, but the rate of propagation of the flame augments much more rapidly in some mixtures than in others. If the tube is a long one the flame will overtake the sound-wave after a more or less prolonged chase, according to the nature of the mixture. But if the tube is short the sound-wave may reach the end of the tube and return as a reflected wave to *meet* the flame, which is still advancing. Our measurements show this to be the origin of the "return-wave" from the end of the tube.

When gases are fired in the centre of a long tube the phenomena of explosion are simple; when they are fired in the centre of a short tube, the sound-waves generated reach the end of the tube before the flames, and the impact of their reflections with the flames produces cross-waves of great intricacy and beauty.

Part VII.—*Further Experiments on the Initial Phases.*

[*In conjunction with* B. Dawson, B.Sc., *and* L. Bradshaw, B.Sc.]

1. *Le Chatelier's Hypothesis of Discontinuity in the Explosion.*

Many photographs show a peculiarity at the point where a less luminous line is succeeded abruptly by a more luminous one. The lines photographed do not appear *continuous*, but the more luminous line appears to start from a point not yet reached by the less luminous one. The point of collision also of two waves appears to project in front of the waves which are meeting. It appeared to me at first as if these appearances might be due to invisible waves advancing in front of the visible ones, but as I found that they only showed where the luminosity of the lines was in marked contrast, and disappeared entirely when the films were not sensitive, or the contrast of luminosity was diminished, I came to the conclusion that the effect was due to halation on the photograph, the brighter lines being enlarged.

But in 1890 Le Chatelier, relying on the same kind of evidence, put forward the view that the wave of detonation starts *in front* of the variable wave (which is increasing in velocity), and originates in an invisible wave which is proceeding in front of the visible wave, and with a velocity equal to it.

This definite judgment of the French experimenter compelled us to re-examine the question. We attempted at first to decide the matter by photographing an explosion as it passed from a less luminous mixture into a more luminous one; but we could not succeed in making the transition sufficiently sudden.

We did, however, succeed in obtaining sudden changes of brightness by introducing a layer of "Welsbach" salts (a mixture of thoria and ceria), and having the rest of the tube quite clean. Although to raise the salt from the glass and to render it incandescent must take some time, nevertheless the photograph shows a small but distinct break in the line of detonation similar to that in question.

It is, of course, easy to show the enlargement due to brightness. If a tube is filled with a mixture giving a luminous explosion, and the explosion is photographed while half the tube is covered over; and if the tube is then filled with a mixture giving a less luminous explosion, which is photographed on the same film while the first half of the tube is covered, a photograph is obtained which shows a greater discontinuity than any of those in question.

Another way of showing the same thing is to photograph a thin platinum wire stretched by weights, and rendered luminous by an electric current. If a second wire is brought to touch the first so as to divide the current, the portion of the wire which carries the whole current is more luminous than the other portion, and the photographs make it appear of far greater diameter.

The evidence against Le Chatelier's view may thus be summarised:—

1. Its supposed effect is only seen when the contrasts are strong, and not on photographs of the same phenomena in which the contrasts are not brought out.

2. It can be initiated in various ways by means of contrasts.

3. The same effect is seen in the collision of two detonation-waves, but Le Chatelier does not suppose that the "invisible wave" can precede the detonation.

2. *Repetition of v. Oettingen and v. Gernet's Experiments.*

The very short time required for the explosion in electrolytic gas to raise the Welsbach oxides to incandescence was strong evidence against the view held by v. Oettingen and v. Gernet, viz., that the detonation of electrolytic gas is invisible, and that the salts present in their experiments only became luminous after the combustion had been for some time complete. Our previous experiments had also shown conclusively that the detonation is not set up *at once*, but only after the flame has run some distance which varies with the nature of the mixture and the position of the spark. But to place the matter beyond all doubt we have repeated their experiments, using a tube of the same size and construction as theirs filled with electrolytic gas, but without the addition of any salts. By careful development the course of the flame can be seen on the negatives from the firing wire. In all cases the explosion begins slowly and has slight luminosity until the retonation-waves are started by reflection from the ends of the tube. Some of our photographs closely resemble in detail those published by Oettingen and Gernet, but they show the initial movements of the flame which are lacking in their photographs. The reflections of the two waves often run nearly parallel, but the stronger wave usually catches the weaker and coalesces with it. The photographs thus make clear how "secondary" waves running parallel with "primary" waves may be produced from a *single* explosion in a short tube.

"On the Measurement of Temperature. Part I.—On the Pressure Coefficients of Hydrogen and Helium at Constant Volume and at different Initial Pressures. Part II.—On the Vapour Pressures of Liquid Oxygen at Temperatures below its Boiling Point on the Constant Volume Hydrogen and Helium Scales. Part III.—On the Vapour Pressures of Liquid Hydrogen at Temperatures below its Boiling Point on the Constant Volume Hydrogen and Helium Scales." By MORRIS W. TRAVERS, D.Sc., Fellow of University College, London, GEORGE SENTER, B.Sc., and ADRIEN JAQUEROD, D.Sc. Communicated by Professor WILLIAM RAMSAY, F.R.S. Received June 19,—Read June 19, 1902.

(Abstract.)

Part I.—By M. W. TRAVERS, *and* A. JAQUEROD.

Pressure Coefficients of Hydrogen and Helium.

The pressure coefficients have been determined by measuring the pressures which the gases exert when the bulb of the thermometer is in melting ice, or in steam at the boiling point. Full details of the method employed are given in the paper of which this is an abstract.

The principal new features of the method are as follows:—The gases were introduced into a glass bulb sealed to a capillary glass stem, which was in turn sealed at the other end to the tube which formed the "dead space"; thus eliminating any chance of leakage of the gas, which must necessarily occur when steel tubes, connected to the glass by cement, are employed. The mercury in the dead space was brought close to, but not into contact with, a point in the dead space, and the pressure on the gas in the thermometer was directly observed by measuring the height of the mercury in an exhausted manometer tube above the mercury in the dead space; the apparatus was so arranged that the two mercury menisci lay on the same vertical axis.

The mercury column and the dead space were enclosed between two parallel glass plates in a water-jacket, the temperature of which could be maintained constant, by means of a rapid current of water, to within 0·02° C. Inequalities in the temperature of the mercury column and dead space, to which the largest errors in such measurements are due, were thus eliminated.

The scale which formed the first surface of the water-jacket was certainly correct to 0·01 mm. The distances between the surfaces of the mercury menisci and the nearest division of the scale were measured by means of a telescope with an ocular micrometer placed at a

distance of one metre from the apparatus. By this means it was possible to obtain readings concordant to 0·01 mm.

In calculating the pressure, corrections were made for capillarity, taking into consideration the height of each meniscus, which was measured at each observation, and for the temperature of the column. The volume of the dead space, which varied with the distance of the mercury in it from the point, and with the height of the meniscus, and the expansion of the bulb with the change of temperature and pressure between 0° and 100° C., were taken into consideration in making the final calculations.

In determining the value of the coefficients, the pressures P_0 and P_{100}, which the gas would exert, supposing the whole of it was at the temperature of melting ice or of saturated steam under normal pressure, were first calculated. Each of the values of P_0 or of P_{100} given in the following table, is the result of four consecutive measurements of the pressure, temperature, etc.

Pressure Coefficient of Hydrogen.

Series I.	(*a*)	P_0	694·458, 694·452
		P_{100}	948·789, 948·824, 941·809
		α	0·00366261
	(*b*)	P_0	696·103, 666·102
		P_{100}	951·059, 951·044
		α	0·00366252
	(*c*)	P_0	706·528
		P_{100}	965·291
		α	0·00366246
Series II.		P_0	520·326, 520·311
		P_{100}	710·897, 710·882, 710·907
		α	0·00366268

Pressure Coefficient for Helium.

Series I.	(*a*)	P_0	690·232, 690·238
		P_{100}	743·044, 943·044, 942·992
		α	0·00366241
	(*b*)	P_0	671·422, 671·418
		P_{100}	917·322, 917·328, 917·352
		α	0·00366270
Series II.	(*a*)	P_0	522·984, 522·984
		P_{100}	714·576, 714·529, 714·577
		α	0·00366313
	(*b*)	P_0	523·016, 523·020
		P_{100}	714·568, 714·583
		α	0·00366255

We have thus three determinations of the pressure coefficient of hydrogen at an initial pressure of 700 mm., to the third of which, since it is the result of one measurement of P_0 and one of P_{100}, and was only carried out to make certain that our instrument was in good working order before determining the pressure coefficient for helium, we attach less weight than to the remaining values. The results of the two experiments with helium are of equal value, each being the result of two determinations of P_0 and three determinations of P_{100}. The mean values of the pressure coefficient for both hydrogen and helium appear to approach 0·00366255, a number which agrees very closely with the value obtained by Chappuis (0·00366254), and is somewhat lower than that given by Onnes (0·0036627) as his final result, though differing little from one of his three actual measurements (0·0036625).

The values of the coefficients at lower initial pressures, do not show the same concordance as those at higher pressure, but they tend to show that at very low pressures the pressure coefficient does not assume a lower limiting value, the reciprocal of which should be the melting point of ice on the absolute scale of temperature. That helium and hydrogen have the same pressure coefficient, and that the coefficient is independent of the pressure, suggest that whatever correction may be necessary to reduce temperatures between 0° and 100° C. on the scale of the hydrogen or helium constant-volume thermometer to temperatures on the absolute scale, it must be very small. Further study of the thermodynamic properties of these gases is necessary for the solution of this important problem.

Part II.—By M. W. Travers, G. Senter, *and* A. Jaquerod.

Vapour Pressures of Liquid Oxygen.

As has been shown in Part I, the coefficients of increase of pressure at constant volume for hydrogen and helium, between 0° and 100° C., have the same value, viz., 0·00366255 or 1/273·03.

Numerous measurements of the boiling points and vapour pressures of liquid oxygen on the constant volume hydrogen scale have been made by previous investigators. The values obtained differ by two or three degrees, and even the most reliable measurements vary between −182°·4 and −182°·7 C. In very few cases are any experimental details given in the original papers; in most cases it is even impossible to ascertain what value was taken for the coefficient of expansion of hydrogen. In every case, however, it appears that the measurements were made by immersing the thermometer in a mass of liquid oxygen, and measuring the pressure under which the liquid was evaporating. As is shown in our paper, it is extremely difficult to maintain liquid oxygen in a steady state of ebullition, and unless a rapid current

of air or oxygen is passed into the liquid it ceases to boil, and may become superheated to the extent of more than one degree. Further, it is very difficult to obtain a sufficient quantity of pure oxygen for such a measurement.

In our experiments a bulb in which a small quantity of pure oxygen could be liquefied was immersed together with the thermometer bulb in a vacuum-vessel containing liquid air or oxygen, through which a fairly rapid current of air was passed. The bulb containing the pure oxygen communicated with the lower chamber of a barometer, so that the measurements of the vapour pressure were quite independent of the atmospheric pressure, with a mercury pump, and with an apparatus for generating pure oxygen from potassium permanganate. Simultaneous readings of this barometer and of the thermometer, which contained hydrogen or helium, gave the vapour pressures of pure oxygen at temperatures which could be varied between 80° and 90° Abs., according as the vacuum vessel surrounding the thermometer bulb, &c., contained freshly made liquid air or nearly pure oxygen.

Four thermometers were employed in these measurements, the capacities of the bulbs being approximately 90 c.c., 12 c.c., 26 c.c., and 27 c.c. Only one single set of measurements on the hydrogen scale were made with the large thermometer, which was the same instrument as was employed in the determination of the pressure coefficients of the gases, for though it was possible to observe a steady temperature by means of it with a degree of accuracy approaching 1 part in 20,000, it was found impossible to maintain so large a thermometer bulb at a constant and uniform temperature, without employing very large masses of liquid oxygen. The results obtained by means of this thermometer differ only by 0·1° from the mean of those obtained by means of the three smaller thermometers. The temperatures observed by means of the three smaller thermometers rarely differ by more than 0·03° from the temperature, corresponding to the same pressures, taken from the smoothed vapour pressure curve.

The form of the thermometers employed in this research, and in the measurements of the vapour pressures of liquid hydrogen, was practically the same as that described in the previous abstract; full details are given in the paper. The pressure on the gas in the thermometer was directly observed by means of a manometer attached to the apparatus, and was independent of the atmospheric pressure. The temperature of the dead space and mercury column were determined by means of mercury thermometers, and the mean temperature of the vertical portion of the stem immediately above the bulb was measured by means of an auxiliary gas thermometer, with a cylindrical bulb of the same length as that portion of the stem of which the temperature is uncertain.

The coefficient of expansion of the glass of which the thermometer

bulbs were made was determined by measuring the contraction of the inner tube of a vacuum vessel, 1000 mm. long, when filled with liquid air. The coefficient between 0° and 100° had been found to be 0·0000284; between 0° and −190° it was 0·0000218.

The pure hydrogen and helium employed in the thermometric measurements was prepared by methods which are described in the next paper, Part III, Appendices II and III.

The results of our measurements show that with constant volume thermometers, in which the pressure at the melting point of ice is about 1000 mm., the temperatures between 80° and 90° Abs. on the helium scale are 0°·1 higher than on the hydrogen scale. Olszewski obtained identical readings on the two thermometers. This may be accounted for by the fact that he employed helium from clèvite which had only been purified by sparking with oxygen, and which possibly contained a trace of argon or other impurity. Our helium was probably quite pure, as it had been passed through a coil cooled to 15° Abs. in liquid hydrogen, and was therefore a more perfect thermometric substance than that employed by Olszewski.

This result is further discussed in the full paper.

The Vapour Pressures of Liquid Oxygen.

Pressures, in millimetres.	Temperatures on the hydrogen scale.	Temperatures on the helium scale.
800	90·60°	90·70°
760	90·10	90·20
700	89·33	89·43
600	87·91	88·01
500	86·29	86·39
400	84·39	84·49
300	82·09	82·19
200	79·07	79·17

Part III.—By M. W. Travers *and* A. Jaquerod.

Vapour Pressures of Liquid Hydrogen.

The liquid hydrogen employed in these investigations was obtained by a method devised by one of us two years ago, and described in the 'Philosophical Magazine,' 1901, vol. 17, p. 412. About 400 c.c. of liquid hydrogen was employed in each of the seven sets of experiments, of which the following are the results. After filling the gasometer with hydrogen, and collecting the quantity of liquid air (8 litres) necessary to cool the apparatus, this quantity of liquid hydrogen can be obtained in half an hour from the moment at which the operations are commenced. Our experience has shown us that when liquid hydrogen is once obtained, it is much more convenient to

manipulate than liquid air. As its latent heat of vaporisation is very high, little loss is entailed through cooling apparatus, previously cooled in liquid air, to its boiling point. Further, the liquid shows no tendency to become superheated, and boils steadily, even under reduced pressure.

Dewar* has obtained the following values for the boiling point of hydrogen on the constant-volume hydrogen scale: – 253°·03, – 253°·37, – 252°·81, – 250°·35; the pressure on the gas at the ice-point being 287 mm., 270 mm., 739 mm., and 127 mm. respectively. On the scale of a thermometer filled with helium containing 7 per cent. of neon, at a pressure corresponding to 728 mm. of mercury at the ice-point, he found the temperature to be 252°·68 and 252°·84 C. In calculating the temperatures on both thermometers he employed Chappuis' coefficient of expansion for hydrogen, 0·00366254.

In our experiments we have employed the three small thermometers which we used to determine the boiling point and vapour pressures of liquid oxygen. The thermometers were filled with pure hydrogen or helium, obtained by the methods described in Appendices II and III to this paper. The small bulb communicating with a manometer, which had in the former experiments contained pure liquid oxygen for the measurement of the vapour pressure, contained, in these experiments, pure hydrogen from palladium. This method of measuring the vapour pressure was essential to the accuracy of the experiments, for it appeared that the vapour pressure of the pure hydrogen, and of the hydrogen in the vacuum vessel surrounding the thermometer, always differed slightly, probably owing to the presence of impurities dissolved in the latter. The agreement between the results obtained with different thermometers containing different samples of gas is indicated in the following table:—

I. Hydrogen Scale.

Thermometer.	Vapour pressures of liquid hydrogen.	Temperature. Found.	Temperature. From smoothed error.
A (12 c.c.).....	757·2 mm.	20°·17	20°·21
B (26 c.c.).....	766·6 „	20·28	20·25

II. Helium Scale.

A (12 c.c.).....	765·0 mm.	20°·42	20°·44
„	759·2 „	20·41	20·41
B (26 c.c.).....	770·0 „	20·43	20·46
C (26·7 c.c.) ...	749·0 „	20·36	20·36

The vapour pressures of liquid hydrogen were measured on the hydrogen scale between the boiling point and a pressure of 100 mm. of mercury,

* 'Roy. Soc. Proc.,' vol. 68, Feb. 1901, p. 40.

and on the helium scale between the boiling point and a pressure of 50 mm., by a method which is described in detail in the full paper. The temperatures on the helium and hydrogen scales were found to differ to a greater extent than at the temperature of liquid oxygen. The difference, as the following table shows, is from 0°·19 to 0°·21 over the range of temperature investigated. Considering that the critical point of hydrogen lies about 35° Abs., while that of helium is probably in the neighbourhood of 10° Abs., this difference is not surprising.

The Vapour Pressures of Liquid Hydrogen.

Pressure in millimetres.	Temperatures on the hydrogen scale.	Temperatures on the helium scale.
800	20°·41	20°·60
760	20·22	20·41
700	19·93	20·12
600	19·41	19·61
500	18·82	19·03
400	18·15	18·35
300	17·36	17·57
200	16·37	16·58
100	14·93	15·13
50	—	14·11

Appendix I.—The melting point of hydrogen was found to be 14°·1 on the helium scale; the temperature given by Dewar* in 1901 is 16°, but an earlier measurement by him† gives the melting pressure as 55 mm. The details of the experiments cannot be entered into in this abstract.

Appendix II.—The pure hydrogen used in our thermometers, &c., was obtained by means of spongy palladium. The method of purifying the gas is given in detail.

Appendix III.—The gas from the Bath wells is not a good source of helium for thermometric purposes since it contains much neon, and the latter, as we shall presently show, has a considerable vapour pressure at the temperature of liquid hydrogen, and cannot be completely separated from the helium.

Pure helium is most readily obtained from clèvite gas, which appears to contain only helium, argon, and a trace of krypton. The gas used in our experiments was passed through a glass coil immersed in liquid hydrogen boiling under normal pressure in one case (thermometer A), and under a pressure of 110 mm. of mercury in another (thermometers B and C). This helium was probably very pure.

* 'Roy. Soc. Proc.,' vol. 68, p. 360.

† 'Nature,' Sept. 21, 1899.

Appendix IV.—The following values have been obtained for the vapour pressures of solid neon :—

Temperature (helium scale).	Pressure (millimetres).
20°·4	12·8
15·65	2·4

The vapour pressure of the neon did not change after successive portions of it had been allowed to evaporate. This proved that neon is a homogeneous substance.

Appendix V.—From consideration of the periodic relationship between the critical and boiling points of the elements of the helium-argon group it appears probable that the critical point of helium lies at about 10°·5 Abs. and the boiling point at 6° Abs.; Dewar fixes* the former of these points at below 9° or 10°, and the latter at about 5°. In a series of experiments helium was compressed into a tube, one end of which was cooled in liquid or solid hydrogen. At temperatures down to that of solid hydrogen evaporating under a pressure of 5 mm. of mercury (probably about 13° Abs.), the pressure on the helium was slowly increased to 60 atmospheres. Under all conditions a change of pressure accompanied a change of volume of the gas, and no evidence that liquefaction had taken place could be obtained.

"On an Approximate Solution for the Bending of a Beam of Rectangular Cross-section under any System of Load, with Special Reference to Points of Concentrated or Discontinuous Loading." By L. N. G. Filon, B.A. (Cantab.), M.A., B.Sc. (Lond.), King's College, Cambridge, Fellow of University College, London, and 1851 Exhibition Science Research Scholar. Communicated by Dr. C. Chree, F.R.S. Received June 12,—Read June 19, 1902.

(Abstract.)

The paper investigates the elastic equilibrium of a long bar of rectangular cross-section in those cases where the problem may be treated as one of two dimensions, namely :—

(*a.*) When the strain being in the plane of xy, the elastic solid extends indefinitely in the direction of z, the applied stresses over the bounding planes $y = \pm b$, $x = \pm a$ being the same for any two sections parallel to the plane of xy. We then have a strictly two-dimensional strain.

* *Loc. cit.*, p. 364.

(*b*.) When, on the other hand, the applied stresses still being in the plane of xy, the thickness of the bar, in the direction of the axis of z, is small compared with the other dimensions of the bar, so that we approximate to the case of a thin plate under thrust in its own plane.

It is shown that, if in case (*b*) we assume (which will be very nearly true, the thinner the lamina) that the normal traction across a face perpendicular to z is zero throughout the thickness, then the equations connecting the *mean* displacements U, V with the *mean* stresses P, Q, S in the plane of the lamina (the mean here being taken with regard to the thickness of the lamina) are of the same form as the equations in case (*a*) connecting the actual displacements u, v with the three stresses in the plane of xy, provided only that we make an alteration in one of the elastic constants, u, v, w being displacements parallel to x, y, z according to the usual notation.

Of course, the lamina being thin, the displacements u, v will probably vary little as we go across it, so that the mean values will give us an approximation to the displacements at every point.

In like manner the stresses in the planes parallel to xy will not differ greatly from their mean values P, Q, S.

The equations used in the paper correspond to case (*b*), but all the results are applicable to case (*a*) by merely changing the elastic constant mentioned. The body stress equations are

$$\frac{dP}{dx}+\frac{dS}{dy} = 0, \qquad \frac{dS}{dx}+\frac{dQ}{dy} = 0,$$

where

$$P = \lambda'\left(\frac{dU}{dx}+\frac{dV}{dy}\right)+2\mu\frac{dU}{dx},$$

$$Q = \lambda'\left(\frac{dU}{dx}+\frac{dV}{dy}\right)+2\mu\frac{dV}{dy},$$

$$S = \mu\left(\frac{dU}{dy}+\frac{dV}{dx}\right),$$

where $\lambda' = 2\lambda\mu/(\lambda+2\mu)$ and λ, μ are the elastic constants of Lamé.

General solutions of these equations are found in terms of conjugate functions. These solutions are then applied to the case of a rectangular bar bounded by the planes $x = \pm a$, $y = \pm b$.

The surface stresses applied to the faces $y = \pm b$ are supposed given at every point, but over the faces $x = \pm a$ only the statical stress-resultants (total tension, total shear, total bending moment) are supposed given.

This last condition is sufficient, provided a is large compared with b. This is assumed in every case; eventually the boundaries $x = \pm a$ are removed to infinity.

The first part of the paper is occupied in establishing the formal

solution for the most general system of applied stress of the above type when a is finite. This is found to lead to infinite series of the form

$$\Sigma (a_n + b_n y) \left\{ \begin{matrix} \cosh \\ \sinh \end{matrix} \right\} \frac{n\pi y}{a} \times \left\{ \begin{matrix} \cos \\ \sin \end{matrix} \right\} \frac{n\pi x}{a} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (1),$$

n being a positive integer, and a_n, b_n arbitrary constants. Together with these infinite series, there enter into the solutions a finite number of terms of the form

$$c_{mn} x^m y^n \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (2).$$

These represent solutions for certain cases where the body stress equations can be solved in finite terms, so as to give zero stress over the boundaries $y = \pm b$. For instance, a uniform tension parallel to x, a uniform bending moment, and a uniform shear give rise to solutions of this type. They can be superimposed upon the others without affecting the stress distributions over $y = \pm b$, and they are introduced to satisfy the terminal "total" conditions.

In the various cases considered, the length a of the beam is allowed to tend to infinity. The series then degenerate into integrals. The transformation and interpretation of these integrals are dealt with at length. It is shown that they may be expanded in series of the form

$$\Sigma (d_n + e_n y) r^n \cos n\phi \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (3),$$

r, ϕ being polar co-ordinates about any point in the beam as origin, n being an integer, and d_n, e_n being constants, which are determined.

The form of these series varies with the origin chosen. When the origin is a point where a concentrated load is applied, the series for the stresses start with a negative value of n, giving terms which become infinite when $r = 0$.

In this case the corresponding series for the displacements contain terms in log r and ϕ, which lead to discontinuities and infinities. These of course could not occur in any actual problem, but in practice the material immediately below a concentrated load would probably become plastic, so that in the immediate neighbourhood of such loads the solution will not apply.

It is found that the terms involving infinities and discontinuities are precisely those to which the solution reduces, when the height $2b$ is made very large. They agree with the solutions given by Boussinesq and Flamant* for two-dimensional strains in an infinite solid bounded by a plane and subjected to load concentrated along a straight line.

The series of terms involving positive powers of r represent therefore the corrections to Boussinesq's expressions, when the finite height of the beam is taken into account.

* 'Comptes Rendus,' vol. 114, pp. 1465—68 and pp. 1510—16.

The various cases, which are separately dealt with, are as follows :—

(1.) When the external stresses upon the top and bottom faces $y = \pm b$ are purely normal and are symmetrical about the mid-section, $x = 0$.

In the first place, making first a large, but not infinite, the various terms of the sine and cosine series (1) may be expanded in terms of y/a. Approximate expressions are then obtained for the displacements and stresses in a very long beam, at a distance from the regions near the points where the loads are applied. The validity of such expansions has been discussed in a paper by the author "On the Elastic Equilibrium of Circular Cylinders under Certain Practical Systems of Load."*

The results in the present case show that to this approximation the stresses P, S are given in terms of the total bending moment and total shear by the formulæ given by de Saint-Venant for a beam terminally loaded, but otherwise free.

In the case of the displacements, however, it is found that, for a doubly supported beam under a central isolated load, the vertical deflection of the central axis contains a term $-kx$ where x is positive, and $+kx$ where x is negative. Such a term was put in by de Saint-Venant for a built-in beam. Professor Love, starting from different conditions for a built-in end, arrived at the conclusion that the term should be zero.

As a matter of fact the term is found to exist, but the coefficient k is only 0·74 of de Saint-Venant's value, showing that in passing an isolated load the slope of the elastic line varies fairly abruptly, but only to about three-fourths of the extent anticipated by de Saint-Venant.

The variations in the central deflection, as the supports are brought closer and closer together, are also investigated. It is found that the excess of the actual over the Euler-Bernoulli deflection (which excess is sometimes referred to by engineers as the "deflection due to shear") decreases eventually as the span decreases and, for exceedingly small spans, may even become negative.

The series in powers of r, deduced from the other expressions when a is made infinite, are used to show the variations of stress in the mid-section and the results are compared with those obtained by Sir G. Stokes† and Boussinesq‡ from an empirical formula. It is shown that, though the empirical formula gives an approximation to the stress in some places, it is by no means to be relied upon.

The case of a beam under two opposite isolated loads, which leads at once to the more interesting problem, of a beam carrying an isolate

* 'Phil. Trans.,' A, vol. 198, pp. 147—233.

† 'Phil. Mag.,' ser. 5, vol. 32, pp. 500—503.

‡ *Loc. cit.*

load and resting upon a smooth rigid plane, is next considered. The distribution of the pressure upon the plane is investigated and a new form of expansion found for it. It is shown that, outside a certain limited area below the load, a *tension* is required to keep the elastic solid in contact with the plane, so that such a solid would be lifted at the sides, by applying pressure at the centre.

(2.) When the stresses across $y = \pm b$ are still normal, but are asymmetrical with regard to $x = 0$.

In particular the behaviour of a beam under two concentrated loads acting in opposite senses upon opposite faces of the beam, their lines of action being on opposite sides of the mid-section, is studied. The manner in which the shear across the middle section varies as these loads are made to approach each other is exhibited by various diagrams. These show how rapidly the effects of the particular distribution of any total terminal load die out as we go away from the end. At a distance of the same order as the height of the beam, they already begin to be negligible.

At a lesser distance than this, however, such effects may become exceedingly important. The case of rivets is instanced, and it is suggested that the results obtained in the paper may give some information which shall be useful in this connection.

(3.) When the stresses across $y = \pm b$ are purely tangential. The special case here treated is that where these stresses reduce to a single concentrated tangential force.

As in practice we cannot approximate to a line distribution of shearing stress, the effect of spreading it out over an area is investigated. It is then found that, though the displacements are everywhere finite and continuous, a discontinuity (though not an infinity) in the surface shear leads to an infinite stress at the point, and is therefore a source of danger to the material.

It is found also that shear depresses those parts of the solid *towards* which it acts. Both these results agree with those previously obtained by the author for circular cylinders.*

The effects of applying tension to a bar by shearing stresses over its faces are considered in this connection. The correction to the readings of an extensometer (which measures the surface stretch), owing to the difference of this distribution of terminal stress from the one usually assumed, is investigated. It is found that no error will be introduced provided no measurements are taken within a distance from the grips less than one and a-half times the long diameter of the section.

Finally the possible cases of solutions in finite terms are discussed, and such a solution is obtained for a beam which carries a uniform load. It is shown that the assumptions of the usual theory of flexure

* 'Phil. Trans.,' A, vol. 198, pp. 147—233.

are in this case no longer true, but are approximately true only if the height be very small compared with the span. The correction to the curvature as calculated from the usual formula is found to be a constant.

The paper concludes with an account and a short discussion of the work of Lamé and Clapeyron, de Saint-Venant, Boussinesq, and, more recently, of M. Mathieu,* M. Ribière,† and Mr. J. H. Michell,‡ which bears upon the subject of rectangular beams. Although, in certain cases, some of the results overlap, the attempt has been made in the paper to co-ordinate them, and to present them in a more complete form, and to develop further the two-dimensional theory, so as to obtain solutions to various interesting questions relating to the effects of isolated loads.

"Antarctic Origin of the Tribe Schœneæ." By C. B. Clarke, F.R.S. Received March 12,—Read April 24, 1902.

[Plate 14.]

The map annexed to this paper is designed to illustrate the geographic distribution of all the species of the Schœneæ—a sub-ordo or tribus of the Cyperaceæ.

The result suggests a flow in geologic time of the sub-order from the South Pole up the three great southern prolongations of land, viz., Oceania, South Africa, Temperate South America; the number of species dying away rapidly as we recede from the South Pole.

I explain how the map is made. I take the outline map of the World divided into twenty-three geographic sub-areas, and my MSS. of the sub-order Schœneæ which show the distribution of every species with reference to these twenty- three sub-areas.

The first species is *Carpha alpina*, R. Br., which I see in the MS. has been collected in the sub-areas 12, 13, 14, 23. I put a spot of black paint in each of these four sub-areas, and proceed to the next species. I have treated 262 species in black dots, two in rings, two in crosses. The black dots do not signify anything as to the abundance of a species; nor in Australia and the Cape do they indicate more than that the species has been collected in that sub-area. But the outlying scattered spots in Central Africa, Japan, Jamaica, &c., are placed as accurately as the scale of the map would admit.

* 'Théorie de l'Elasticité,' Paris, 1890; also 'Comptes Rendus,' vol. 90, pp. 1272—74.

† 'Sur Divers Cas de la Flexion des Prismes Rectangles,' Bordeaux, 1889; also 'Comptes Rendus,' vol. 126, pp. 402—404 and 1190—92.

‡ 'Quart. Journ. Math.,' vol. 32.

All the material on which the map is founded has been seen and determined by me; no fact in the distribution is copied out of a list or depends on a name or a distribution number. The material recorded by number or name of collector in my MSS. is about 1850 collections. The great mass of the commoner species, or commoner habitats, is not recorded in my MSS., but is virtually included in the map.

The genera included in this sub-order are very close together, and may be arranged on a different system from mine; this would not affect at all the map, which deals only with species. I have doubtless made some errors in the specific determinations. Moreover, any competent man revising the material would have a different opinion from myself as regards some species and varieties. The utmost alterations that could thus be necessitated in the map would be two or three black dots more (or less) in Australia and the Cape.

The rings represent *Schœnus nigricans*, Linn., a cosmopolitan species, and *Schœnus ferrugineus*, Linn., a species closely allied to it but confined to the sub-area 1—"Cooler Europe." [It is so closely allied as to be sometimes confounded with it by learned cyperologists.]

In an exactly parallel manner, the crosses represent *Cladium Jamaicense*, Crantz, a cosmopolitan species, and *Cladium triglomeratum*, Nees, a United States plant so closely allied that some competent cyperologists call it a var. of *C. Jamaicense*.

Five of the genera occur in Oceania and South America; two occur in South Africa and South America; five occur in Oceania and the Cape; but the genera are so closely allied that little can be inferred from this. Only two or three species are common to Australia and South America.

Numerical tabulations on a large scale are viewed with suspicion by botanists, as they are often drawn in great part from books or from lists; the percentage of errors then introduced from wrong identifications, diversities in nomenclature, variable limits assigned to species and areas, and doubtfully wild species, is so large that it invalidates the conclusions. No one of these sources of error taints the present tabulation.

The conclusions suggested are—

(1.) The sub-order Schœneæ originated in prehistoric time at some centre on which the three streams of species (Patagonian, Cape, Australian) converge, and has spread from that centre northward.

(2.) Two of the genera, viz., Schœnus and Cladium, have developed largely, and produced each a great number of species.

(3.) One species in each of these two genera has become cosmopolitan, a common case in large genera from one end of the Genera Plantarum to the other.

(4.) Each of these two cosmopolitan species, superabundant in individuals, has thrown off a local offshoot, which in A.D. 1902 produces in our minds the impression of a geographic sub-species.

The whole preceding argument hangs on the question whether I have included the right genera in my sub-order Schœneæ. The Schœneæ dealt with in this map, are the 3-style-branched Schœneæ of Bentham, which are regarded as a very closely allied group by Kunth, Boeckeler, F. Mueller, and the orthodox cyperologists. In the Plant-families of Engler and Prantl a different system has been adopted; this, however, has not been accepted in their writings on Cyperaceæ either by Germans as Goebel, Solms-Laubach, Celakovski, or by Americans as Britton, Bailey. The present map is grounded on the systematic arrangement of Cyperaceæ by Bentham.

The few points in the map which I regard myself as doubtful I have given against my own case, *e.g.* :—

The outlying species marked in the North-west Himalaya is a small species—an elementary form—which I have placed in the Schœneæ, but may possibly really belong to some other sub-order.

Several outlying localities in West Africa and the West Indies, belong to one genus—*Remirea*—which was regarded by Bentham as an *abnormal* member of the Schœneæ.

There are several, and some large, orders of plants, as Proteaceæ, Restiaceæ, &c., confined, or nearly so, to the Southern Hemisphere; the present case is only one branch of a very large argument, presented in detail.

EXPLANATION OF MAP (PLATE 14).

The map represents the world-distribution of the 266 species which constitute the Schœneæ, with reference to the 23 sub-areas lined out in black. For each species a mark is put in each sub-area in which it occurs.

For 262 species the mark is a black dot.

For the two species *Schœnus nigricans*, Linn., *Schœnus ferrugineus*, Linn., the mark is a ring.

For the two species *Cladium Jamaicense*, Crantz, *Cladium triglomeratum*, Nees, the mark is a cross.

SCHOENUS.
FERRUGINEUS, LINN.
NIGRICANS, LINN.
CLADIUM.
JAMAICENSE, CRANTZ.
TRIGLOMERATUM, NEES.
REMAINDER OF THE SUB-ORDER i.e. 262 SPECIES.
T P Collings Sc

"On the Structure of the Gills of the Lamellibranchia." By Dr. W. G. Ridewood. Communicated by E. Ray Lankester, M.A., F.R.S. Received May 3,—Read May 29, 1902.

(Abstract.)

This paper records the results of an investigation undertaken at the instance of Professor E. Ray Lankester, F.R.S., and carried on under his supervision. 215 species of Lamellibranchia, belonging to 118 genera, were examined. The results are far-reaching in that they demonstrate that, except in a very broad way, the minute structure of the gill, like the grosser structure, cannot be taken as an indication of genetic affinity.

Three main types of gill structure can, however, be recognised, representing apparently three successive grades of complexity. The first type, found in the families Nuculidæ and Solenomyidæ, is distinguished by the mutual freedom of the gill leaflets or platelets into which the embryonic gill papillæ expand. These two families were united by Pelseneer under the heading "Protobranchia," which title it is proposed to retain.

In the other two types the embryonic papillæ elongate into filaments, which are held in juxtaposition by interlocking cilia disposed in circular patches on the anterior and posterior sides of the filaments, or by regularly arranged horizontal bars of cellular tissue, which put the adjacent filaments into organic connection the one with the other, and convert the interfilamentar spaces into rows of fenestræ. The former type it is proposed to term *eleutherorhabdic*, and the latter *synaptorhabdic*.

Although Pelseneer so far recognised the distinctness of these two last conditions as to make them the prime features of his orders Filibranchia and Eulamellibranchia, he yet instituted an intermediate heterogeneous order—the Pseudolamellibranchia—to include the Ostreidæ, Aviculidæ and Pectinidæ. Evidence is produced in the present communication to show that the order Pseudolamellibranchia is ill-founded, and based largely on a misconception of the relative value of the flatness or plication of the gill lamellæ, and the presence or absence of large filaments (principal filaments) at regular intervals along the lamellæ.

In the family Solenidæ particularly it is shown that different species and sub-genera of the same genus may have their gill lamellæ flat or plicate, and the filaments all of one kind or with enlarged principal filaments at intervals.

Avicula argentea has proved to be a form of supreme interest, in that it demonstrates how, in some cases at least, the synaptorhabdic

condition has been evolved from the eleutherorhabdic condition, since in this species both ciliated discs and organic interfilamentar junctions are present.

Anomia aculeata is of no less interest, since it differs from the other species of *Anomia* examined, and resembles the rare *Dimya*, in that the gill filaments are not reflected, *i.e.*, they have no ascending portions.

A very careful examination was made of the gills of *Vesicomya* and *Euciroa*, which were said by Dall to exhibit close resemblances with those of the Protobranchia. Both prove to be of the synaptorhabdic type, and their superficial resemblance to the Protobranch gill is due to an expansion of the interlamellar edge of the filaments, a feature which is by no means confined to these two genera.

Evidence is also adduced to show that the forms included by Pelseneer in his order Septibranchia are, at least so far as can be judged by their branchial organs, degenerate Molluscs of the *Lyonsiella* type, and the suppression of the Septibranchia as a distinct order is advocated.

"On some Phenomena which suggest a Short Period of Solar and Meteorological Changes." By Sir Norman Lockyer, K.C.B., F.R.S., and William J. S. Lockyer, M.A., Ph.D., F.R.A.S.

Received June 14,—Read June 19, 1902.

In continuation of the inquiries referred to in a former paper on Indian rainfall and solar activity,* attention has more recently been devoted to an examination of the variations of pressure over the Indian and other areas.

1. It is well known that in India during the summer months (April to September) and during the winter months (October to March) low and high pressures respectively prevail. In the case of the latter, the pressure is found to exhibit very remarkable and definite variations, and is in excess, every $3\frac{1}{2}$ years, on the average, while at these times of excess of high pressure the low pressure during the other 6 months of the year is deficient; so that every $3\frac{1}{2}$ years or so the high pressure becomes higher and the low pressure is not so low as usual.

2. Further, this short-period variation, which appears in the mean variation of pressure over the whole of India, is as well defined in the mean values for individual stations, such as Bombay (fig. 1, Curve F), Calcutta, Madras, Nagpur, &c.

3. The view that the variation of pressure in question over India and its neighbourhood is not due to local causes, but is produced by some

* 'Roy. Soc. Proc.,' vol. 67, p. 409.

external or extra-terrestrial action, is considerably strengthened by an examination of the pressure-curve of a very distant station, such as Cordoba. Dealing with the pressure of Cordoba during the high-pressure

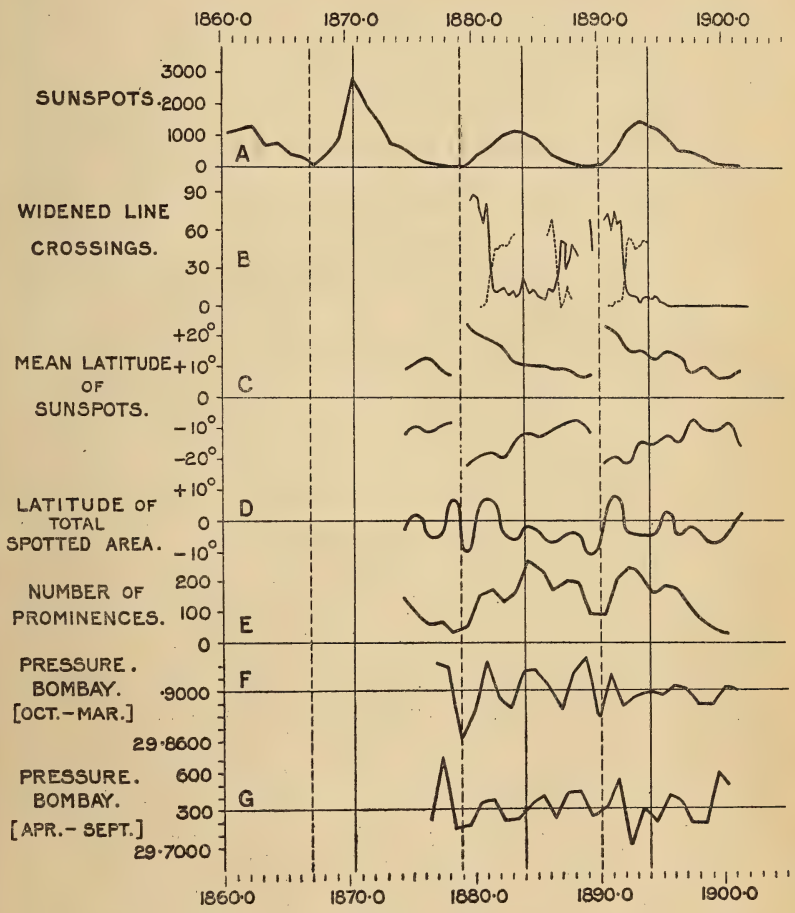


FIG. 1.

months, April to September, the curve (fig. 2, Curves F and E), representing the variation from the mean from year to year, is exactly the *inverse* of the curve representing the Bombay and other Indian pressures for the same months over the same period of time. The cause, therefore, which raises the mean value for the low-pressure months over the Indian area would appear to lower the mean value of high-pressure months at Cordoba simultaneously. In fact, we have a see-saw.

4. Further investigation shows that not only do the pressures of

practically the whole Indian area exhibit variations from year to year, which present very similar features, but that this is the case with other large areas. Thus, for instance, it is found that the yearly mean pressures for Brussels, Bremen, Oxford, Valencia, and Aberdeen (the only pressures that have been at present examined) are all remarkably similar in their variations from year to year, and it might almost be said that one curve, representing the variations from the normal, would approximately define the pressures at all these places.

The probable extra-terrestrial origin of these short-period variations led to a detailed examination of the records of the phenomena connected with solar spots and prominences, with a view of seeing whether similar variations, indicating changes in the solar activity, could be detected.

5. A preliminary reduction of the Italian observations of prominences observed on the sun's limb since 1871 was first undertaken. The result of this inquiry indicates that, in addition to the main epochs of maximum and minimum of prominences, which coincide in time with those of maximum and minimum of the total spotted area, there are prominent subsidiary maxima and minima having a similar short period and also coinciding in time. (Fig. 1, Curve E.)

6. Although these subsidiary prominence pulses are not distinctly duplicated in the curve representing the spotted area of the solar surface, it is to be noted that corresponding pulses are indicated in the curves which represent the change of latitude of spotted area from year to year; and in each case an increase in prominence activity is associated with a decrease of latitude of the spotted area. (Fig. 1, Curves C and D.)

7. A comparison of these solar data with those already referred to relating to terrestrial pressures suggests that these simultaneous outbursts of prominences and changes of the latitudes in which the spots occur about every $3\frac{1}{2}$ years are the true cause of the pressure changes; and that the varying intensitv of solar activity during the sunspot period of 11 years produces an effect on the pressure and circulation of our atmosphere, thus affecting the whole globe meteorologically.

8. The close correspondence between the epochs of these subsidiary pressure variations and those representing prominence frequency, suggests not only their very close relationship, but that the terrestrial pressure quickly answers to the solar changes, while so far as the work has gone it would appear that rainfall (fig. 2, Curves A, B, C, D) and snowfall are subsequent effects.

9. It may be remarked that we have already obtained evidence showing that this short-period variation is not the only one acting, but that the 11-year and 35-year periods apparently influence the short-period variations; but even this does not explain some anomalies already met with, and should the solar origin of these short-period

pressure changes be subsequently confirmed, some of them not constant in all localities will have to be explained; and it is possible we may obtain in this way some new knowledge on the atmospheric circulation.

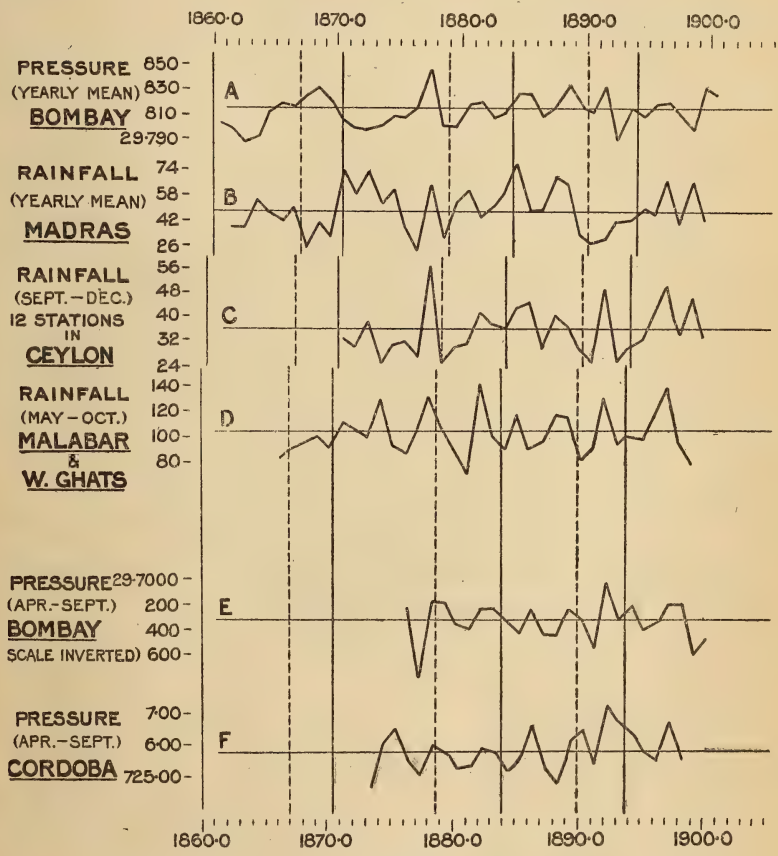


FIG. 2.

10. The period of time included in this survey begins generally with the establishment of the full records of the Indian Meteorological Department in 1875 and extends to 1895, when the regularity of the widened-line phenomena was broken, as stated in a previous communication.

ADDENDUM. Dated June 26.

In continuing the above researches we have plotted the percentage frequency of the solar prominences derived from the Italian observations for each 10° of solar latitude N. and S. of the Equator.

We find that the epochs of maximum prominence disturbance in the higher latitudes are widely different from those near the Equator. The latter are closely associated with the epochs of maximum spotted area; the former occur both N. and S. at intervening times.

We have then two sets of strongly marked prominence outbursts occurring at intervals of between 3 and 4 years.

Both sets are represented closely in the Indian pressure curves.

"On Two Methods for the Limitation and Regulation of Chloroform when administered as an Anæsthetic." By A. Vernon Harcourt, F.R.S., Reader in Chemistry at Christ Church, Oxford. Received June 17,—Read June 19, 1902.

In the 'Transactions of the Chemical Society' for 1899 an apparatus is described for providing a current of air mixed in any desired proportion with chloroform vapour. This apparatus served its purpose in experiments on small animals, but was on too small a scale, and offered too much resistance, to provide the free supply of air at a rate of 4 or 5 litres a minute which human respiration requires.

A gas passing close over the surface of a liquid for a sufficient distance yields to the liquid any part of the gas which is soluble, and takes up from the liquid any part of the liquid which is volatile, as completely as when the gas bubbles through the liquid, while suffering much less resistance to its passage.

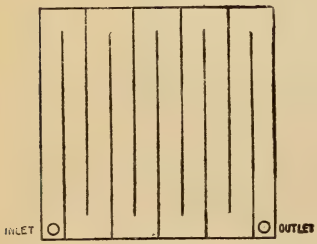


Fig. 1.

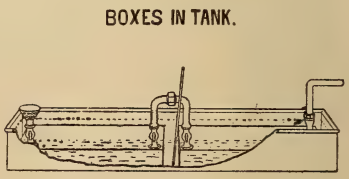


Fig. 2

The apparatus shown here (figs. 1 and 2) consists of two closed boxes made of galvanised iron, a square foot in area and 2 inches in height, with transverse partitions soldered to the top and reaching nearly to the bottom. The partitions are $\frac{3}{4}$-inch apart, and extend from one side to within an inch of the opposite side each way alternately. Large stopcocks at the two ends of one of these sides provide inlet and outlet. By this arrangement the gas travels in each box a distance of about 15 feet over the surface of the liquid.

The outlet of box I is connected, by an arched tube joining the stop-cocks, with the inlet of box II. Air is drawn in at the inlet of box I, and air and chloroform are drawn out from the outlet of box II. The first box is charged about half-full with a mixture of chloroform and alcohol, the second with water, about $2\frac{1}{2}$ litres in each. The two boxes stand in a shallow tank, and are immersed in water of which the temperature can easily be kept constant. The second box, with its charge of water, is for the purpose of absorbing alcohol vapour. When the mixture of air with the vapours of chloroform and alcohol has passed slowly for several hours, the water takes up no more chloroform; but the absorption of alcohol vapour continues almost indefinitely.

By varying the proportion of chloroform to alcohol and the temperature of the mixture, the proportion of chloroform to air in the gas drawn through may be varied from a large percentage to a small fraction per cent. If the apparatus is on a sufficient scale relatively to the maximum volume of gas drawn through in unit time, the rate of passage of the gas will not affect the composition of the mixture, saturation under the actual conditions having been attained.

The following experiments were made to test the fulfilment of the last-named condition of constancy:—

(1.) Air was drawn from the boxes through a flask for a quarter of an hour at the rate of 400 c.c. per minute. The contents of the flask were then analysed; percentage of chloroform, 1·45.

(2.) Air drawn through for 8 minutes at the rate of 4000 c.c. per minute; percentage of chloroform, 1·42.

(3.) Repetition of (1). Passage of air and chloroform at 400 c.c. per minute, continued for half an hour. Percentage of chloroform 1·47.

The temperature of the water in the tank was throughout 9° C.

The difference in the proportion of chloroform when air is passed through at 400 and at 4000 c.c. per minute is too small to be material. Hence, in the apparatus employed, the length of travel of the air over the surface of the liquid is sufficient for the vapour of chloroform to have attained at the greatest rate of passage, being that of ordinary respiration, its maximum tension at the actual temperature and partial pressure upon the surface of the liquid.

The method used for estimating chloroform is that in which a flask having a capacity of 800 or 900 c.c., of which the interior has been wetted with a few c.c. of water, is charged with the mixture of air and chloroform (fig. 3). There is then brought into it a spiral of fine platinum wire, through which an electric current can be passed. The water at the bottom of the flask is heated to between 50° and 60°, and the wire is rendered incandescent. A cloud of hydrochloric acid appears, which dissolves in the water. In 20 minutes the change is complete. The

flask is cooled, the cork withdrawn, and more water poured in. Finally, the acid liquid is neutralised with a standard solution of ammonia. The capacity of the flask being known, and the temperature and atmospheric pressure when it was filled having been noted, the percentage of chloroform in the mixture of chloroform vapour and air is readily calculated.

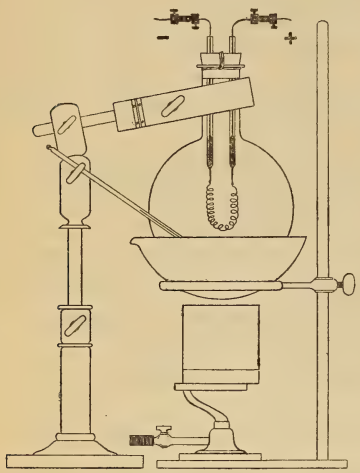


Fig. 3.

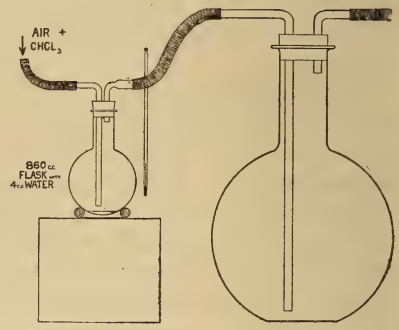


Fig. 4.

Many such analyses have been made in the course of this inquiry, the flask being charged by taking the inspirations of ordinary breathing through it for a couple of minutes. To save the operator from being chloroformed, a flask of 25 litres capacity was interposed between the smaller flask and the mouth (fig. 4).

For the analysis of smaller quantities of air and chloroform than will suffice for filling a flask by displacement of air, and for determining the total amount of chloroform dissolved by a liquid, such as water or blood, another apparatus has been devised, the chemical reaction being the same. It consists of a tube about 20 cm. in length, and from 2 to 2·3 cm. in diameter, near the axis of which are two fine platinum wires attached at one end to a stouter wire passing through the glass, and at the other to the shorter arm of a lever turning upon another wire at right angles to it which passes in through the side of the glass tube; the longer arm is weighted so as to fall when the wire is elongated by heat and keep it stretched (figs. 5 and 6). When the electric current, from wires attached to the platinum terminals outside the tube, is turned off, the lever moves in the opposite direction. The tube is set slightly aslope; from its lower end a tube descends into a flask, in which the hydrogen chloride formed is collected; at the upper

end enter the mixture of chloroform and air, and also water falling into a lateral tube at the rate of about one drop a second. The slow trickle of water along the bottom of the tube is heated by the wire above it, and thus furnishes the necessary supply of steam, while serving also to dissolve the hydrogen chloride, and carry it into the flask beneath.

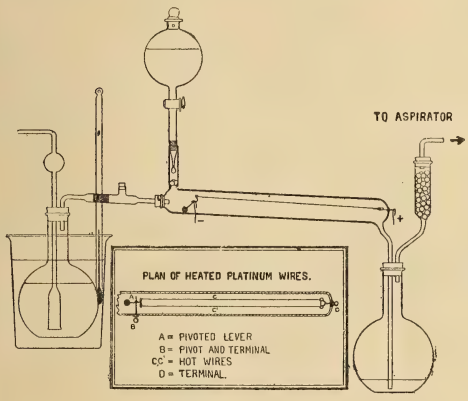


Fig. 5.

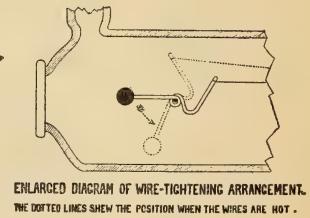


Fig. 6.

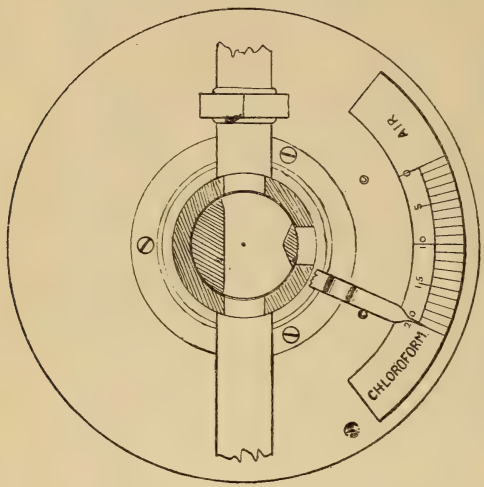
A full account of these methods of testing with the record of the experiments by which their trustworthiness has been established, will probably appear in the 'Journal of the British Medical Association,' for a Committee of which body the second method has been planned and used.

By the first method it has been found that to obtain a mixture of 98 per cent. air and 2 per cent. chloroform, which suffices to produce full anæsthesia, the box should be charged with a mixture of 80 per cent. alcohol and 20 per cent. chloroform and kept at a temperature of 15°. With this mixture, and at this temperature, however slow an inspiration may be, the proportion of chloroform to air cannot rise above the comparatively safe limit of 2 per cent.

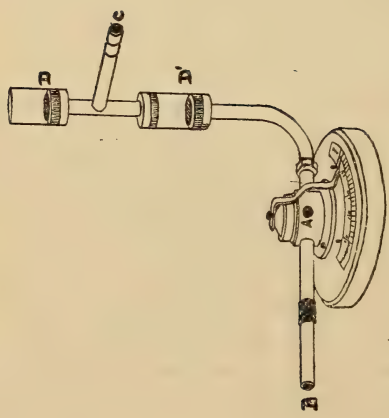
To reduce the dose of chloroform below 2 per cent. a three-way stopcock has been made giving always free passage to air, but admitting at one end of its range air with the full charge of 2 per cent. of chloroform, at the other end air only. Between the two is a graduated arc over which a pointer moves, the divisions on which give approximately the percentage of chloroform in the air which is being inhaled. The long handle of the stopcock which forms the pointer is easily moved by the pressure of the finger.

At the outlet of the stopcock are two valves, opening and closing with each inspiration and expiration respectively. Between the two valves is a short tube connected by a piece of flexible tubing with a face-piece of the usual type (fig. 7).

In this country, though not in Scotland, chloroform has been largely superseded by ether and nitrous oxide. But the author hopes the apparatus he has described may be of use in hospitals in which chloro-



Cross-sectional plan of stop-cock. Full size.



A = Hole to admit air.
B = Entrance tube for chloroform vapour.
C = Inhalation tube.
DD' = Sheet rubber disc valves.

FIG. 7.

form is used, and may perhaps help towards restoring confidence in chloroform as a safe anæsthetic.

Except for hospital use, a more portable apparatus seemed desirable, and the author was also urged by a surgeon of experience to connect the stopcock and valves with the face-piece, so that they and the face

of the patient might be observed together. The second apparatus is the result of an attempt to accomplish these objects (fig. 8).

The limitation of the maximum amount of chloroform is effected by restricting the area from which evaporation takes place, all the air required for respiration being drawn through a small two-necked bottle half-filled with chloroform. It might be thought that in this case the percentage of chloroform mixing with the air would vary greatly with the rate of passage of the air. But this is not so. When air enters rather slowly it passes down one neck and across to the other over the surface of the more saturated air with only partial admixture. When air is drawn through in larger volume it blows down through the neck and stirs up and mixes with the vapour beneath, promoting evaporation, and by this means compensating for the greater dilution. Thus with this apparatus also, using a bottle about 3 cm. in diameter, and maintaining a temperature of 16°—18°, the maximum amount of chloroform is about 2 per cent. Further dilution with air to reduce this percentage is obtained by the use of a stopcock similar to that already described. The temperature of the chloroform is observed by means of two specific-gravity bulbs, which are in neutral equilibrium in chloroform at 16° and 18° respectively. When one of these bulbs has sunk, and the other is floating, the temperature of the chloroform is between 16° and 18°, a variation of temperature which, it is thought, will not make a material difference. By evaporation the temperature falls, and it can be restored by dipping the bottle for a few moments in warm water. In this case also the readings of divisions on the arc represent approximately the percentage of chloroform which is being administered at any moment.

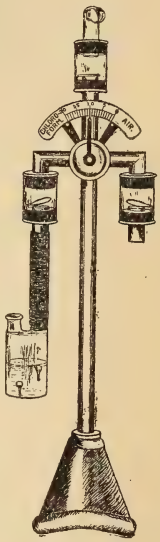


FIG. 8.

The apparatus is provided with three valves, the movement of which supplies information as trustworthy as readings of the stopcock. The upper valve gives passage to all expired air, and from its movement are seen the fulness and frequency of respiration. The height to which the valve connected with the bottle of chloroform rises shows how much air with 2 per cent. of chloroform is being inhaled; the height to which the valve admitting pure air rises shows to what extent the mixture of air and chloroform is being diluted with pure air. For example, when the pointer is in the middle of the arc the two valves rise equally, showing that the original dose is being diluted with an equal volume of air, or that there is being administered air containing 1 per cent. of chloroform.

Instead of attaching the face-piece to the stem of the apparatus, a piece of flexible tubing may be interposed, the apparatus being supported on a stand.

For the illustrations of apparatus and help in the work the author is much indebted to Mr. A. Angel, B.A., Christ Church.

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